PROCEEDINGS

OF

THE ROYAL SOCIETY

OF

EDINBURGH.

VOL. VIII.

NOVEMBER 1872 TO JULY 1875.

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ROYAL SOCIETY OF EDINBURGH.

VOL. VIII. 1872–73. No. 85.

NINETIETH SESSION.

Monday, 2d December 1872.

SIR ROBERT CHRISTISON, Bart., President, in the Chair.

The following Council were elected:—

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SIR ROBERT CHRISTISON, BART., M.D., D.C.L., LL.D.

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HIS GRACE THE DUKE OF ARGYLL.

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Prof. A. DICKSON.	GEORGE ROBERTSON, Esq., C.E.
JAMES LESLIE, Esq., C.E.	Captain T. P. WHITE.

Monday, 2d December 1872.

Sir Robert Christison, Bart., the President, read the following Opening Address:—

The Society now enters into its 90th session since its foundation. During the session recently concluded the number of its Ordinary Fellows has increased from 331 to 343. Twenty-two members were admitted during last session. In the twelve months ending with the 30th ultimo, death has laid his hands sparingly upon our brethren. During the former year we lost by death ten Ordinary and three Honorary Fellows, several of whom occupied during their lives a very high position in science. Last year has deprived us of only four Ordinary Fellows, and one on our Honorary list; and although these were all men of great estimation in their several professions, our Honorary Fellow alone, among them all, was widely known as a successful scientific labourer. Therefore the duty I have now to discharge as their biographer is evidently on two accounts a brief one.

The Ordinary Fellows who have disappeared from amongst us during last year are, Dr Thomas Barnes of Carlisle, Dr Patrick Miller of Exeter, Dr John Addington Symonds of Clifton, and the Right Reverend Charles Hughes Terrot, Bishop of Edinburgh in the Scottish Episcopalian Church. Our only deceased Honorary Fellow is Hugo von Mohl, Professor of Botany in the University of Tübingen.

The three first were graduates of the University of Edinburgh, each of them distinguished during a long life by his successful cultivation of medicine, and the pursuit of medical practice in an important district of England. It has often been remarked that there is scarcely in all England an important county town, where for almost a century past the leading physician of the town and surrounding country has not been a graduate of the University of Edinburgh. Drs Barnes, Symonds, and Miller illustrate a fact which the University has always regarded with allowable pride.

Dr Thomas Barnes was born in 1793, near Wigton, in Cumberland. He commenced his medical studies, according to the fashion of the time, by becoming apprentice to a medical practitioner in that town; where, among other duties, he had to supply physic to horse, dog, and cow, as well as to the human subject, to sell pepper and coffee, and to look after his master's Rosinante. Having thus cultivated medicine in a practical way, he proceeded, in the inverse order, to study the principles of medicine and its fundamental sciences in the University of Edinburgh at the age of eighteen. In more recent times medical students carry on their studies differently. Apprenticeship is at a sad discount with them. They study instead literature, philosophy, and physical science, then begin medicine at the root of the tree, and end their school studies with that of medical and surgical practice. It was different in the time of Dr Barnes' noviciate; even in my young days, two-and twenty-years later, I was almost a solitary example of an unapprenticed British student, working till the age of eighteen in the Faculty of Arts and at the Natural Sciences, instead of wasting the most precious of his years in dispensing drugs, and practising physic before learning it; and, indeed, there are even still some estimable men, *laudatores temporis acti*, who sigh over the now fast vanishing old rule, and lament the disappearance of medical apprenticeships, and the loss of apprentices.

Dr Barnes, after studying medicine for six years, partly in this University and partly in London, Paris, and Germany, took his degree at Edinburgh in 1817. Soon afterwards he settled as a consulting physician in Carlisle, where at first he made only that slow progress to which those must usually submit who choose this the highest class of medical practice,—but where before long he became for many years the leading physician over a great extent of the northern counties of England. In this pursuit he led a life of great professional activity and usefulness till his 56th year, when failing health led him to restrict his field. At the same time, he did much good to the place of his residence by founding some, and encouraging and improving other, important charitable establishments. Nor did he neglect the improvement of medical knowledge. For, though he never contributed any large work to

the literature of medicine, he communicated from time to time to the professional journals many papers of acknowledged value.

Like most county and country practitioners of ability, he found recreation in cultivating a favourite branch of science, little, if at all, connected with his profession. This in Dr Barnes' instance was meteorology. His studies in this department of natural history were the main cause of his being associated with us as a Fellow of the Royal Society. A paper by him on "The Meteorology of Carlisle for 24 years," was read to the Society in 1830; and in the same year he was elected one of our Fellows. Forty years later, and therefore quite recently, he contributed to the Proceedings of the Society a continuation of his inquiries on the same subject.

Dr Barnes died in March last, in the 79th year of his age.

Dr John Addington Symonds, another county physician of great eminence in England, was born in 1807 at Oxford, where his father practised the medical profession. The opportunities of his birth-place gave him the inestimable advantage of an excellent general education, which ever afterwards shone out in his tastes and the occupations of his leisure hours. After taking advantage for two years of the limited opportunities which Oxford in these days presented for the study of the fundamental sciences of medicine, he repaired in 1825, being then in his 19th year, to the University of Edinburgh, where he graduated three years afterwards. He was a very distinguished student, as I personally know, for he was one of my pupils in 1827, when I was Professor of Medical Jurisprudence. Nor were his studies confined to medicine,—philosophy at that time being also a favourite pursuit. Returning to Oxford, he assisted his father in his practice for three years. But in 1831 he was induced to settle in Bristol; and there at first, and afterwards in conterminous Clifton, he passed his whole professional life.

Dr Symonds attained great reputation as a practising physician at an early age. Crowds were attracted to him from all quarters of Great Britain, partly no doubt by the salubrity of the climate of Clifton, but chiefly by the eminence of its physician. The climate had been long considered favourable for the treatment of

certain common pulmonary diseases. Taking advantage of this peculiarity, Dr Symonds, while neglecting no corner of the field of medical practice, studied with great care and success the diseases of the lungs. In consequence, the suffering members of the community flocked to him in great numbers from all parts. Many went to him from Scotland, many from Edinburgh. Among the latter Principal Forbes, while Professor of Natural Philosophy, repaired to Clifton at my recommendation; and he derived so much benefit from his first residence there, under the care of Symonds, that he was able to struggle bravely with his malady for many years, and at one time indeed seemed as if about to shake off his deadly enemy altogether. Forbes often spoke to me with admiration and gratitude of the Clifton physician's kindness, skill, and philosophical acuteness; and it is no wonder that two such men continued fast and intimate friends ever after.

Local celebrities, in positions similar to that of Dr Symonds, not unfrequently owe success to qualities different from those which ought alone to give a title to it. Dr Symonds was none of these, but in every sense of the word a true physician of the purest dye, of excellent talents, rare assiduity, deep discernment, well-balanced determination, unpretentious bearing, thorough conscientiousness in every thought and act, a sincere unostentatious Christian,—in short, a man qualified to rise to a high rank wherever he might have chanced to choose his path in the medical profession.

But Dr Symonds was more than a physician. He had a fine taste for art. His classical training in youth led him to keep up familiarity with ancient literature. Philosophy, too, was his frequent recreation from professional toil. It is no wonder, therefore, that, when his part of physician had been discharged and came to an end, that of friend continued; and that he thus enjoyed through life the friendship and society of not a few of the most remarkable men in British literature, science, and politics during the last thirty years.

These his tastes are displayed in the subjects he chose for those of his writings, which have been collected and published since his death by his son. He has contributed less than he might have done to the literature of the practice of medicine in

the strict signification of that term, though his experience eminently qualified him to illustrate it. His works in the posthumous collection of them consist of Essays on topics of a general nature, generally delivered as addresses to public meetings, and the titles of which prove the nature of his favourite pursuits, and the variety of his endowments. They treat of the Principles of Beauty, of Waste, of Decades of great events in the world's history, of Knowledge, of Sleep and Dreams, of Apparitions, of the Relations between Mind and Muscle, of Habit, of the Criminal Responsibility of the Insane, of the Public Estimate of Medicine, of the Health of Clifton, of Medical Evidence; and the last of these treatises is an address on Health, delivered at the Bristol meeting of the Social Science Association in October 1869.

There is none of these able essays, distinguished alike by sound sense, ingenious views, logical discussion, and purity of style, which might not be analysed here with credit to his memory, and advantage to those now listening to me. But I must remember that brevity is the first essence of this Presidential Address; and, therefore, I shall confine myself to a single paper, probably indeed not the most attractive of them for the general reader, but which illustrates well Dr Symonds' ability as a statistical inquirer and critic. In an address delivered at Bath, he tells us that "a severe shock was inflicted on the sanitary sensibilities of Clifton," by the town being gibbeted in the "Times" newspaper, on the authority of the Registrar-General's mortality returns, as "the most mortal of watering places,"—because, while at Torquay, Cheltenham, the Isle of Wight, &c., the annual death rate in every 1000 of the population ranged from 15 to 17 only, in Clifton it mounted so high as 24. The truth, as demonstrated by Symonds, is an admirable illustration of the frequent fallacy of statistics, and the danger when rash, ill-trained minds dare to deal with them. Dr Symonds shows that the supposed high death-rate is founded on a single quarterly return, while the returns taken from other quarters of the same year, or from other years, vary so much as from 24 down to so low as 14·8 in 1000 of the population; and he shows further that the returns made use of in the "Times" do not apply to Clifton the watering-place at all, but that the newspaper writer had committed the ridiculous mistake of con-

founding with it the "Clifton Union," a term including a far more extensive and populous district, from which the watering-place is sharply defined by site as well as by structure, and which adds to the count a great mass of the most needy population of the Bristol suburbs. Thus, raking away the rubbishy statistics of the newsmonger, he proves by irrefragable facts, on a scale of several annual returns, that the yearly mortality of the justly famous watering-place is no more than 17 in 1000.

Dr Symonds published, in the professional periodicals chiefly, various valuable papers on various strictly practical subjects, which it would be out of place for me even to enumerate here, and much more so to discuss. They were received as they successively appeared with approbation by his professional brethren, and may be perused now with profit by every professional student.

In 1854 Dr Symonds became a Fellow of the Royal Society of Edinburgh, led to us partly by his early connection with the University, and partly by the close ties of friendship contracted with many Scotsmen, of whom, or of whose relatives, he had been at Clifton the skilful and sympathising physician.

In the autumn of 1868 his health for the first time began to fail, and, though he took early warning and contracted greatly the field of his labours, his ailments grew upon him, and proved fatal on the 25th February 1871.

Dr Patrick Miller, another eminent physician who practised during a long life at Exeter, died there in December 1871, in his 90th year, Senior Fellow of the Royal Society of Edinburgh, and also, I apprehend, Senior Graduate of the University of Edinburgh, where he took his degree as Doctor of Medicine in 1804. I cannot find satisfactory evidence that any graduate of that year, or of those immediately preceding it, has survived him. In our own list there still stands one name for the year 1818, in which Dr Miller was elected a Fellow; but for some time past, on careful inquiry, no trace can be discovered for identifying this his contemporary.

Dr Miller was connected with us by ties dear to every member of our Society. He was grandson of the celebrated Professor of Mathematics in the University, Dr Matthew Stewart, and nephew

of that Professor's still more famous son, Professor Dugald Stewart. He was educated here at the High School and University, under the eye, and very much in the family, of his uncle; a precious privilege, which, among other advantages, secured for him the early, and throughout life uninterrupted, friendship of several of the greatest men in the subsequent history of our country, such as Lords Palmerston, Lansdown, and Brougham, who were his uncle's pupils.

Three years after graduation he settled in Exeter, where he was so well received, that in two years more he was appointed physician of the Exeter Hospital for the sick. He soon became the leading physician of the town and adjacent country;—a position for which he was eminently qualified by his large, highly-cultivated mind, his courteous, kindly, gentle manner, combined with great energy and decision, high professional attainments, and a strong, robust, healthy frame and constitution. He was also constantly employed in doing good in other ways in the city of his adoption. He was an original founder, or active promoter, of every one of the various public institutions, organised in his lifetime at Exeter, for the bodily care and mental culture of the labouring classes, in whose welfare he constantly took great interest.

In these days of rapid development and change in the medical sciences, and in the details of medical practice, there are not many men of Dr Miller's unquestionable ability, who, in professional positions parallel to his, have not contributed by their writings to some branch or another of professional progress. But Dr Miller's tastes did not lie in the direction of medical publication, although his wide and long experience must have supplied him with great store of materials.

Until eight years before his death, Dr Miller continued to retain great bodily activity. Rheumatism, however, then gradually circumscribed his powers in that respect; but the faculties of his mind, even his memory, are stated by one who knew him well, to have been preserved unsubdued till very near the close of his life.

He repeatedly visited Scotland and Edinburgh, to keep alive his old associations; but for many years past I fear he must have encountered very few to recall to him his old associates. His regard for his University was shown a few years ago by his pre-

senting to the University collection a valuable bust of his uncle, Dugald Stewart, by the sculptor Joseph; who has produced an excellent likeness of the philosopher, as I remember him on the only occasion when I ever saw him, in advanced old age, in the Edinburgh theatre, at one of the early representations of the late Mr Murray's famous dramatic conversion of "Rob Roy."

CHARLES HUGHES TERROT, Bishop of Edinburgh, was a descendant of one of the many French families which the revocation of the Edict of Nantes drove from their native land to seek a home in this country. His father entered the Indian army, and was killed at the siege of Bangalore a few weeks after the birth of his son, which took place on the 19th of September 1790. To his mother accordingly fell the charge of young Terrot's education, and we may attribute to the influence of her powerful mind much of her son's subsequent eminence. Mentally as well as personally there was a striking resemblance between the two.

Before sending him to Cambridge, Mrs Terrot placed him with one of the good men of his day, the Rev. John Fawcett of Carlisle. In 1808 he entered Trinity College, Cambridge, where, as he was wont to say, he obtained his real education by daily converse with such men as Whewell, Peacock, Rolfe, Amos, Mill, and Robinson, especially the last two, with whom his intercourse ceased only with life. He took his degree in 1812, obtaining a position on the Honour list altogether inferior to what his subsequent appearance as a mathematician would have warranted us in anticipating. The fact is that Terrot's mind revolted at the drudgery of acquiring branches of the science towards which he felt no inclination. It was characteristic of him to tread a small circle, but to tread it well; and he was constitutionally unfitted for stowing away in his memory, for temporary purposes, facts and figures in which he took no interest. Thus his degree examination resulted in a comparative failure. Nevertheless, on the Fellows of his college this failure made no impression. They had enjoyed ample opportunities of judging of his accuracy and of his acuteness, and they did not hesitate to elect him into their body in the very year in which he took his degree. He did not retain his Fellowship many years, having married in 1815, and settled in

Haddington. The leisure of a country incumbency permitted his entering the list of competitors for University honours of a literary kind. In 1816 he obtained the Seatonian Prize for a poem on the Destruction of Sennacherib's Army before Jerusalem.

To have been the author of a successful prize poem in the University of Cambridge is not a little honourable, though it must be confessed that secular themes have enlisted a higher display of genius on their side than sacred. Witness such names as William Whewell, Thomas Babington Macaulay, and Alfred Tennyson. Still Terrot's poem is very far from being an ordinary production. Portions of it indeed are deserving of a high rank, and as a whole it is striking and effective. The finest part is the night scene, in which is depicted the Assyrian army encamped before the walls of Jerusalem, waiting with feverish anxiety the first streak of dawn to commence the assault. The author introduces the reader to a humble tent, in which lie two soldiers, restless and tossing through the whole night. Each dreams his dream. The one, eager for battle,

> "Dreams that with Jewish blood his spear is red."

He has cleared the ramparts, and with his comrades is rushing wildly on the devoted city. The other, "of softer mind," is carried away to the home of his affections on the banks of the Tigris,

> "To the rude cot where dwelt his infancy."

He is welcomed back by his friends "with a smiling tear,"

> "And she whom best he loves, who loves him best,
> Hangs round his neck, and weeps upon his breast."

The pleasant dream is broken by the frantic struggles of his comrade. He awakes,

> "And fear comes over him,—he knows not why."

The curtains of the tent are shaken;

> "a blast
> From heaven moaned low and sadly as it passed."

It is the "icy wind of death." On that blast rides the avenging angel, carrying "the last long sleep" to all that slept that night in the Assyrian host.

The success of this his first poem seems to have inspired Terrot with the ambition to try a more difficult theme. In 1819 he published anonymously a poem, with the rather prose-inspiring title, "Common Sense." In this production the poets and politicians of the day were pretty freely criticised by a hand which wielded some of the power of the Dunciad and the Rolliad combined. That the poem was vigorous and pointed no one who knew Terrot can for a moment doubt. But it was not the less a great mistake in the author to attempt to weigh poets in the balance of common sense; and the attempt accordingly failed. A line or two from the first page may be taken as a specimen:—

> "Time was when bards were few: then might you see
> In Button's room the whole fraternity.
> But now, like Egypt's frogs, on every hand
> They spread, and croak, and darken all the land."

As a poet, then, it is clear that Terrot would have found himself in very unpleasant society. He accordingly renounced the fraternity, and carried his common sense, of which he had an abundant stock, to the regions of theology and mathematics. With his theology we have in this place no concern. But a very graceful recognition of its merits has appeared in the sketch of Terrot's life from the pen of Dean Ramsay, who has delineated his character by a few well-marked and kindly touches.

Our concern is only with his mathematics. To mathematics, when harassed by the cares and vexations incident to his position, he had recourse as a retreat from irritating thoughts. His passion for this science was strong enough to take possession of his mind, and soothing enough to settle it down to repose. Bishop Terrot contributed several papers to the Transactions of this Society. The subjects treated of were the Properties of Numbers; the Square Roots of Negative Quantities as Symbols of Direction; and the Theory of Probabilities. To the papers on the second and third of these subjects it may be permitted to make more than a passing allusion.

In January 1847 he read to the Society a paper, entitled, "An Attempt to Elucidate and Apply the Principles of Geometry, as published by Mr Warren in his Treatise on the Square Roots of Negative Quantities." The subject here treated of had been

floating somewhat dimly before the eyes of mathematicians for half a century, and was just then beginning to assume a living form in the mind, and a living exponent, though a somewhat obscure one, in the writings of Sir W. R. Hamilton. It was not until six years later that the doctrine of Quaternions of the great master, as developed in his "Lectures," swallowed up in its vast amplitude all that had preceded it. Terrot must accordingly be considered as one of the pioneers of the science. In the paper now referred to he points out the applicability of the method to plane trigonometry in all its parts; but he could see his way no further. Years after, when paralysis had laid him low, on being told that Symbols of Direction had been embodied by Sir William Hamilton into the full-grown science of Quaternions, his delight was expressed in the form of thankfulness that enough of life had been spared him to know that the dream of his early years had been realised, even although all power to comprehend it had passed away from him.

In 1858 Bishop Terrot published in our Transactions a paper "On the Possibility of Combining two or more Probabilities of the same Event, so as to form one Definite Probability." This paper was his best contribution to mathematical science. In addition to its own excellence, it has the merit of having drawn forth the valuable paper of the late Professor Boole, "On the Application of the Theory of Probabilities to the Question of the Combination of Testimonies or Judgments," to which the Council of this Society awarded the Keith medal in 1858. In this paper the conclusions of Bishop Terrot are confirmed, and a flood of new light is cast on the subject. It ought perhaps to be added, that an extended correspondence between the Bishop and Boole had preceded the publication of the papers in question; in which the Bishop had steadily manifested an anxious desire both to promote the advance of science, and to aid Boole in his upward career. Selfish ends had no place in the Bishop's mind.

In dismissing this brief notice of Bishop Terrot's scientific connection with the Royal Society, it may not be amiss to add a word or two on his personal connection with us. For many years of his life he was one of the regular attendants of our meetings; and when not actively engaged in the work going on, he was an

attentive listener, and, when occasion called for it, an unsparing critic. He had a real love for the Society. As he left the building for the last time, he expressed himself to the effect, that thenceforth his heart would be with us, but that the work of his hands was done. The only part of our proceedings which he did not relish was the tea-drinking after the meeting.

What the Bishop was in private it is for others to tell. Dr Hannah, one of his most intimate friends, testifies of him, that he rejoiced in conversation, and never tired of it so long as, in his own phrase, "the talk was good;" and that, with the keenness of his wit and the quickness of his repartee, he united tolerance and good nature. Dr Fawcett, who also, when a medical student, knew Bishop Terrot well, says, "his manner was short and abrupt, but he was always spicing it with something good." Not a few members of this Society can likewise bear testimony to his wonderful felicity in conversation. But we are now more concerned with the impression which he made on society at large. He was there eminently conversational. He did not talk much; but he talked well. He had the faculty of saying powerful things in a few pithy, pointed words, which always hit, and generally remained fixed in the mind. His humour was dry, even caustic; but neither personal nor ill-natured. His criticisms of authors were sometimes severe, but they were never meaningless. For example, of one of Goethe's later works of fiction, which to ordinary minds appears wild and extravagant, Terrot was wont to say, that Goethe, having during a long life inhaled incense from the worshippers of his genius, had in his old age become satiated, and accordingly gave the world what he knew to be worthless, in order that the admiration it should call forth might ascend as pure incense direct to himself.

This remark of the Bishop's, whatever it may be worth, will help us to get a faint glimpse at a prominent feature in his character as a man. The feature in question was a dread for himself and a dislike in others, of appearing to assume that to which they had no just title,—of seeking out the upper chambers,—even of claiming a place to which the world at large would raise no objection. This feeling rendered him sensitive as regarded himself, and critical in his remarks on others. But his judgments were

tempered with so real an insight into character, so just an appreciation of all that was worthy, and withal were so free from the suspicion of envy or jealousy, that they never produced a rankling sore or gave rise to a bitter repartee.

The Society will kindly treat with indulgence this imperfect attempt at the portraiture of one of the most noteworthy of those whom death has recently removed from among them.*

Hugo von Mohl, the only Fellow whom death has struck off our Honorary list during the last year, was long eminent among the botanists of the Continent for his researches in Botanical Physiology. In his student days medicine was his main pursuit, but combined with the ardent cultivation of botany and geology. He graduated with great distinction at Tübingen, and was encouraged to make medicine his profession by his father, who filled an important office in the Würtemberg Government. But the son's bent was turned more and more to botanical investigation, which by degrees became his great object in life, to the utter disregard of medical practice. He entered on his task with the great advantage of a mind highly cultivated in the collateral sciences, as well as in the languages. With the further advantage of a robust frame and constitution, he was enabled to make at an early age frequent successful excursions in his own neighbourhood, and also in the Alps, gathering extensive collections of plants, and accumulating materials for future study. He then commenced his researches into the anatomical structure of the Palms, Ferns, and Cycads. In his twenty-sixth year he was appointed Sub-director of the Imperial Gardens at St Petersburg; next year, without having taken up that office, he was elected Professor of Physiology in the Academy of Bern, and then in the University of that city; and in his thirtieth year he was promoted to the Chair of Botany in his own University of Tübingen. Eight years afterwards, on account of his services to science, he was raised by the King of Würtemberg to the rank of nobility. A few years later, in spite of his apparently robust constitution, he became subject to catarrhal affections. Although he succeeded

* For the preceding sketch of Bishop Terrot's life, the Society and I are indebted to Professor Kelland.

in throwing off this enemy, he subsequently suffered from pleurisy, and also from liver complaints. Again restored to health for some time, he was seized, in May 1871, with obstinate giddiness, which, although it disappeared and left him apparently well, was nevertheless the presage of his end; for, on the 1st of April last, he was found dead in bed, having to all appearance sustained an attack of apoplexy during the night. He died in the 67th year of his age.

Mohl published his researches chiefly in the form of occasional papers or monographs. He is the author of two books only, the one on Micrography, the other on The Vegetable Cell. But his occasional papers are no fewer than ninety in number, the most remarkable of which belong to the domain of Vegetable Histology—the earliest and most important being his treatise *De Palmarum Structura*, published in 1832, in his twenty-sixth year. Many of his writings relate also to Vegetable Morphology and Botanical Geography, and some to Botanical Physiology. In every branch his researches display much originality, and have added materially to the structure of modern botanical science.

Professor von Mohl was a tall, strong man, a bachelor, reserved in manner and disposition, of retired and somewhat peculiar habits, in all things conscientious and upright, free altogether from vanity, regardless of all consequences in upholding the truth, entirely devoted to scientific research.

Having now discharged to the best of my ability, within the space to which I am confined by the necessary limits of this address, the duty owing to the memory of our Fellows who have been removed from among us by death during the twelvemonths just concluded, I do not know that I can apply the rest of your time this evening better than by referring to the present position of certain scientific proceedings in which the Society takes an interest.

The first subject I shall take the liberty of bringing under your notice is the present condition of the Ordnance Survey of Scotland.

I am almost afraid to say—in the year 1872—when the Government Survey of our division of the United Kingdom was commenced; but it had made some progress when I witnessed a demonstration, by the chief surveying engineer, of the construc-

tion and powers of Ramsden's great theodolite, stationed on the Calton Hill in the year 1817 or 1818. Now in the present year, as we may gather from the catalogue of published maps which appeared by authority on the 8th of May last, there remains to be published about two-fifths of the sheets of the 25-inch survey; fully one-half of those on the 6-inch scale; and of those on the 1-inch scale—the scale most generally desired by the public at large—no less than two-thirds of the whole. In the course of a life which has not been short I have witnessed the completion of one-third part of the Survey. At this rate, some grandson of the youngest among you, if he be fortunate enough to attain a great age, may be also so lucky as to see the whole maps before he dies; and yet I cannot guarantee even him that pleasure.

To the 1-inch map of Scotland the index map of the May Report assigns 120 compartments or sheets; but Orkney and Shetland, which are left out, will require three more. Of the whole number, only 44 were published on the 8th of last May—38 of them shaded, and 6 in outline. These embrace all Scotland south of the Forth and Clyde, and, to the north of that boundary, the counties of Fife, Clackmannan, Stirling, and Dumbarton, most of Perthshire, all Forfar and Kincardine, a little corner of Aberdeen, the island of Arran, and, far apart from all other completed work, the "Ultima Thule" of the west, the island of Lewes. Since 8th May there have been issued separate slips, showing what has been added since, viz., two sheets, one of which is an outline map of a small portion of the coast line of Aberdeenshire, and the other the small corner of that county already mentioned, with a conterminous part of Kincardineshire, converted from outline into a shaded map. The Society will judge for themselves how much remains to be done, and what is the rate of progress of the 1-inch maps.

The misery of the want of two-thirds of these maps is enhanced by the minute accuracy and admirable execution of those which we do possess. Permit me to illustrate this statement by a single incident. This was no more, indeed, than an incident in the holiday life of a wanderer in quest of recreation; but numberless analogous occurrences must happen to others engaged with more important objects. Four years ago I made a long day's excursion

from Arrochar, with an English friend, round the base, up to the summit, down again a great way, then up again over a lofty spur into an upland valley, of one of the neighbouring mountains, Ben-Arnen; which is very seldom visited, although it is very interesting in structure, and 3050 feet in height, and commands a magnificent view in all directions. Descending into the heart of the valley, in which there are many fine precipices, we twice came suddenly near the brink of these, as a stranger is apt to do in going down hill on such mica-slate mountains; but the instinct of experience forewarned us of our approach to danger, and enabled us to avoid it by a flank route. On returning to town, I tried to trace this excursion on the best of our ordinary maps, but in vain; for in some our mountain was not to be seen at all, while in others it was put in evidently *ad libitum*, and in not one was it named. In the Ordnance shaded 1-inch map, however, every valley, every spur, ravine, grassy slope and precipice is given so precisely that I am sure I could furnish any stranger to the mountain with a route upon that map by which he could safely follow our track. All praise, therefore, to Sir Henry James and his faithful assistants, who could little have thought that their work, in so remote, wild, and little known a corner, would be subjected to such minute criticism from so improbable a quarter. It is not in his department that the blame lies for the hideous delay in the progress of the Ordnance Survey of Scotland, and for our not having long ago reaped all the advantages of its completion. Very far from it. But what are we to say of the blindness, and deafness, and misplaced economy of successive Governments, who, possessing such an admirable instrument as the Ordnance Survey Office, refuse to make use of it, to the full extent of its power, in one of the most important and most attractive of all branches of civil administration? And what has become of the nobility, gentry, men of science, and others in Scotland, who in former days did not sit so tamely under disregard of their just claims upon the State?

The publication of the maps upon the 6-inch scale is somewhat farther advanced. These include, besides the country mapped on the 1-inch scale, all Perthshire, most of Aberdeen, all Banff and Nairn, Cantyre, and the southern half of the other peninsula of

Argyllshire, which is bounded by Loch Long and Loch Fine, with the island of Bute.

The maps on the 25-inch scale are advanced still farther, especially when it is considered that this large scale is not applicable to a great extent of mountainous, unproductive land throughout the Highlands and Islands of Scotland. In northern parts they include, besides the counties mentioned above as portrayed on the 6-inch scale, that of Elgin, a third part of Inverness, most of the Argyllshire mainland, but none of its islands; and, very far north indeed, the Survey now extends to a small patch comprising the central parish of Watten in Caithness, which thus hangs "en l'air," far remote from every other indication on the index map of Ordnance Survey operations. Very singular are the omissions in the more southerly counties. Fife is altogether excluded; so is Kinross, and so are Mid-Lothian and East Lothian, four of the most purely agricultural counties; to which must be added the more chequered shires of Kirkcudbright and Wigtown. Perhaps these rich districts are already so far provided with every desideratum which an accurate and minute survey is intended to promote,—roads are so abundant and perfect, railways so numerous, water-supply so complete, field-drains so perfect, estates so well surveyed by their possessors,—that such counties may be left by Government to look after themselves. But there should be better reasons, I imagine, for districts of so great importance being left so long unprovided with that scale of survey and map for which they are peculiarly fitted.

A single word more on this subject. How is the 25-inch survey to be made accessible in Scotland? By individuals purchasing such of the separate maps as they severally need? But there are various professions whose members may require to consult very many, and to have access, at one time or another, to all. But no such individual can afford to pay L.1500, the price of a complete set of 25-inch maps, or the space for preserving them conveniently accessible. It would surely be no unreasonable demand on the parental care of Government that a complete set should be made accessible to the public at Edinburgh, Glasgow, and Aberdeen. I understand that some such boon has been asked for, but declined.

I have here pointed out a line of action in which the Royal Society may usefully exert itself. It has, indeed, done so without avail before now; but that was a number of years ago. Government may at last be roused to do justice if repeatedly appealed to; and it should be remembered that we have to knock at a door, which in general must be well battered before it can be opened.

Having considered the present occasion an apt one for reminding you, and through you the public at large, of the great desire expressed by this Society about eighteen months ago to obtain a thorough catalogue and scrutiny, and general concurrence in the preservation, of the most remarkable boulders in Scotland, I have asked the chairman of our Boulder Committee whether he could supply me with any information for the Society as to the progress made in this matter since the printing of the very full and able report of the Committee last April. Mr Milne Home agreed with me that the time and occasion are opportune, and has therefore kindly furnished me with some interesting particulars and general views, which I am sure you will approve of my having elicited, and which I shall now present very much in his own words.

Mr Milne Home continues to receive from abroad assurances of warm sympathy on the part of Continental associations engaged in the same work. He has not yet received, in reply to the invitations issued by our Committee, any communications from geologists and others, who, in their wanderings last summer and autumn, must have had opportunities for adding to the Committee's stock of facts. But may I not hope that this appeal may even still elicit a favourable reply? In the meanwhile Mr Milne Home has himself acquired, by his personal exertions, so much new information, that we shall scarcely feel this year the want of communications from others.

Desirous of carrying through in some measure the inspection of known boulders asked for by the Committee, Mr Milne Home made a tour "through the districts indicated by the following towns, viz., Callender, Aberfeldy, Pitlochrie, Dunkeld, Perth, Forfar, Aberdeen, Forres, Elgin, Nairn, Inverness, Tain, Kingussie, Lochaber, Fort William, Glenelg, Tyndrum, and Killin.

"Many of the boulders in these districts are entered in the list of the Committee's Preliminary Report; but Mr Milne Home also fell in with many others which will be detailed in the next Report. The present sketch will be mainly confined to some points bearing on the probable mode of transport of the boulders.

"1. The first inquiry was the quarter whence the boulder had come, when the rock composing it was different from the rocks of the adjoining district. In all the districts visited the parent rock seemed situated in a direction between north and west from the boulder. This fact did not surprise me in regard to those in the counties of Stirling, Perth, Forfar, and Kincardine;—situated as they were principally in the low grounds south and east of the Grampians, which undoubtedly produced them. These boulders had probably come down the valleys. But boulders were also found in the counties of Moray and Nairn, which apparently had come from the same direction, viz., from points between north and west. Here the same explanation was impossible; for they must have travelled across a considerable extent of sea. In these two counties, there are boulders of granite, gneiss, and a very compact conglomerate, which came most probably from Caithness, Ross, and Cromarty; and besides these rocks, of which great mountains exist in the north-west, there are to be seen smaller boulders of oolite,—a rock forming a narrow fringe along the eastern shore of Ross and Caithness.

"This point being of some importance with reference to the mode of transport, one or two other facts may be mentioned which seem to confirm the conclusion that the boulders of Moray and Nairn had come from the north-west. 1. The rocks of the hills on or near which they lie, had manifestly been shaven, ground down, polished, and scored by some powerful and wide-spread agent passing over them from the same direction. 2. In most cases the boulders lie on hill-slopes facing the north-west, as if arrested in their farther course by the high ground. I could not help concurring in the remark of a farmer, who was pointing out to me four or five huge boulders on the same hill-slope, that 'in takkin' the hill, they had stuck on it." 3. In most cases the boulders, when long-shaped, lie with their longer axis in a north-west direction, and also with their sharper end towards

the same quarter, as if moved into that position by some agent which had been flowing past them. These facts seem to indicate conclusively, that some powerful agent had passed over this part of the earth's surface, crossing what is now an arm of the sea, the Moray Firth, carrying great masses of rock, and dropping them at considerable distances.

"In the counties of Moray and Nairn, the boulders are at all heights, from the sea-level close to the shore, up to the height of about 500 feet. But in other districts they are to be seen as high up as 2500 feet above the sea. Many of them are perched on hill-tops, or very near the tops, and many are in such positions as to indicate that, whatever was the transporting agent, they could not have fallen from any height. These positions are rocky hill-sides, where the slope is so considerable that the boulders could easily have slid down with a very small amount of force applied.

"The angular form of the boulders is also instructive. Thus there is one huge cubical block of old conglomerate on the border of Nairn with Inverness, called "Tom Riach," to which Captain White first called attention, each side measuring almost exactly 21 feet. It lies on nearly horizontal beds of Old Red Sandstone, in a wide valley, with no cliff near it. There can be no doubt that this boulder, weighing betwixt 600 and 700 tons, must have been brought from a great distance—and otherwise than by rolling or pushing, because, from the sharpness of its angles, it evidently had undergone no friction. There are hundreds of boulders, which, lying on the open surface of the country, sometimes on bare rocks, sometimes on gravel deposits, give similar proofs that they must have been transferred by some agent, without friction. The boulders referred to, are generally single; but there are two districts where they are huddled or grouped together in such a way, as to indicate that they had been all brought to the spot by one transporting agent which went no further in its forward course. One of these places is to the south of Inverness, at or near the mouths of two valleys which unite at their lower ends. It is just beyond the mouths of these two valleys that the boulders occur in enormous numbers, composed of rocks existing in the valleys, to the west and north west. Another place is Lochaber, where there are long thick lines, or trainees, of boulders, forming parts of a semicircle or horse-

shoe, the concave sides facing a valley, from which the boulders appear to have issued. In this locality there are also elongated mounds of rubbish, running more or less parallel with the lines of boulders,—very similar to the moraines so common in Switzerland. The impression made on the mind by an inspection of these two localities was that the transport of the boulders found there was due to glaciers.

"There is a third class of boulders, distinguished from the two classes just referred to. The latter are generally angular, and lie on the upper surface of the land. The third class are rounded in shape, and imbedded in gravel or clay. They are, in short, huge pebbles, having evidently undergone tremendous friction by being pushed or forced along an uneven surface, in contact with other stony materials.

"Some of the boulders belonging to the first and third classes have been carried great distances; and when it is considered that they had to pass across valleys, ranges of hills, and arms of the sea, the difficulty of the problem as to the mode of transference is vastly increased. For example, there are in the county of Berwick several granite and mica-slate boulders, which,—if they came from the Highland hills, as they probably did,—must have crossed many ranges of hills, and at least one arm of the sea, and one large valley, that of the Firth of Forth.

"Until many more facts have been ascertained, it would be a pity to form very decided opinions as to the agency of transport. Instructive as some districts are among those referred to above, there are others probably even more so on the west coast of Scotland, and on the Hebrides. It is desirable that the boulders reported from these quarters, should be visited scientifically; for in size and peculiarity of position, they are said to be even more remarkable than those now described. The Boulder Committee have in their custody schedules representing the place, size, and other particulars of these boulders, which they will lend willingly to any geologist who will inspect them and report on them.

"The Committee had two duties assigned to them. Besides ascertaining the position of remarkable boulders, they were to endeavour to secure the preservation of the most interesting. They have not yet proceeded to the fulfilment of this second duty,

except in a single case, where, at the special request of a parish minister, they applied to the proprietor, on whose lands the boulder lay, to prevent the destruction of it by the tenant; and this application proved successful. When the Committee proceed further in discharge of the same branch of duty, they may experience some difficulty. It may, therefore, be not out of place to state now what has been done by the Boulder Committees of France and Switzerland on this point.

"These Committees have adopted several plans of conservation. In some cases, they have acquired a right of property in the boulder, by means of a regular deed, signed by the proprietor of the land. In some cases, the proprietor has granted this right only for his own lifetime. The identification of the particular boulder was matter of difficulty; but this has been got over by describing the land on which it stands, and cutting out on one of its sides the letter F for France, or S for Switzerland.

"The success of the Swiss Committee has been most gratifying. In the Canton of Soleure upwards of 200 boulders have been secured from destruction,—one of these being a magnificent block at Steinhof, weighing about 5000 tons. It was purchased by the Communal Council for L.16, and given to the Natural History Society of Soleure. The famous 'Pierre à Bot,' near Neufchatel, a granite boulder from Mont-Blanc, weighing about 2000 tons, now belongs to the Communal Council of the Canton. The blocks of 'Monthey,' which Principal Forbes described in this Society, have been gifted by the proprietor to the Helvetic Society of Natural Science. From the list appended to Professor Favre's Fourth Report of last year, it appears that the Swiss Committee have succeeded in insuring the preservation of several hundred boulders;—not all of gigantic size, but each interesting for some other reason, such as position, historical association, or traditionary name or legend, or for having been made triangulation points by a government survey, or marking the boundary between parishes or cantons, or because named after distinguished alpine travellers, such as Charpentier, von Buch, and Venetz.

"It is interesting to see how cordially the objects of the Swiss Committee are sympathised with, not only by the government, local as well as general, but likewise by the people at large. Pro-

fessor Favre mentions in his last Report, that the Town Councils of Bienne, Bondry, and Soleure, and the Cantonal Councils of Berne, Friburg, Aargau, and Neufchatel have aided the Committee in various ways; and in a previous Report, he stated that the public purse had been freely opened to defray the expenses of the Committee.

"It would not be right to conclude without adding, that the Swiss Committee in their last Report have been pleased to take favourable notice of our own similar movement in Scotland, observing that it has received not only the support of the Royal Society of Edinburgh, but likewise the approval of the British Association for the Advancement of Science, and that the course of proceeding in Scotland is the same as that followed in Switzerland.

"Whether our Committee will adopt the Swiss plan of acquiring a right to property in any of the Scottish boulders is a question for consideration. Already good has been done by the inquiries which the Committee has instituted, and by their explanation of the scientific value and historical interest of the boulders; a disposition to preserve them has been thereby created which did not previously exist. The press has also noticed with approval the appointment of our Boulder Committee, and has no doubt influenced public opinion."

The Society will have no difficulty in perceiving with what view I have given on the present occasion this detailed communication from Mr Milne Home to me. I trust that the public may be encouraged to aid in the preservation of our boulders. I hope that geologists will without delay aid the Committee in visiting and investigating them. And it may be a question whether our own Council may not consider that they could scarcely expend more profitably a portion of our moderate funds, than in sending out some young but competent geologist to some of these distant parts of the country indicated by Mr Milne Home, where there are remarkable boulders, which have not yet been described or investigated, or even scientifically visited.

In the address delivered to the Society at the opening meeting in December last, I brought before you some observations on the

Temperature of the Water at Great Depths in Loch Lomond, as exemplifying that of the deep fresh-water lakes of Scotland generally. I afterwards communicated other observations made in the middle of April last, on the first approach of weather warmer than that of the preceding winter months; which, however, were unusually open and free from frost. The result was that between the middle of September 1871, to the middle of April 1872, in parts of Loch Lomond, varying between about 500 and 600 feet in depth, there is constantly at the bottom a great sheet of water from 250 to 350 feet in thickness, the temperature of which remained steadily at 42°, whatever might be the temperature of the surface-water, or that of the air immediately over it. I beg now to supplement these observations very briefly with a few made since, in continuation of them.

But allow me, in the first instance, to do justice to others who had previously made observations somewhat similar, and whose results were last year imperfectly, and some of them altogether, unknown to me.

So early as 1767, Horace Benedict de Saussure made thermometrical observations in the lake of Geneva, finding the temperature at 82 feet to be 55°·6, when at the surface it was 78°. This was in the middle of August.

In 1774, Mallet and Pictet, in a deeper part of the lake, opposite the Castle of Chillon, found at a depth of 300 feet a temperature of 51°, while that near the surface in August was 76°. This result, says Saussure, "is very remarkable; for 51° is two degrees and a half below the mean temperature of the earth at Geneva."

De Saussure afterwards extended his researches greatly. But, in the first place, not being acquainted with any available register-thermometer for such observations, he laboured to construct one which should retain, when hauled up, the temperature it had attained at the bottom. He at last succeeded in constructing such an instrument by using a thermometer whose bulb was an inch in diameter, surrounding it with a non-conducting coat of wax, resin, and oil three inches thick, encasing the whole in a wooden box, two-thirds of an inch in thickness, and securing the whole with tight iron ferrules. His instrument, which was thus a cylinder above seven inches in diameter, had the lamentable defect of

requiring to remain twelve hours at the bottom, to arrive at the temperature of the surrounding water. But the zeal and patience of the philosopher were a match for this trial, and his construction had probably the advantage of securing his bulb and tube against the disturbing influence of pressure, which must have been great in some of his experiments, but which, as he never refers to it, must not have occurred to him as a condition to be provided against. He then, between 1779 and 1784, made a number of observations on the lakes of Geneva, De Joux, Annecy, Thun, Bourget, Brienz, Lucerne, Constance, and Lago Maggiore; always reaching the bottom at depths varying from 80 English feet to 163, 240, 335, 350, 370, 500, 600, 620, and 950 feet. His observations were generally made at midsummer, a few in February, and a few also in October. Excluding the experiment in Lac de Joux, whose depth of 80 feet excludes it from the category of deep lakes, and that of Maggiore in a warmer latitude and locality than the Swiss lakes, we find that he never got a higher temperature than 42°·1, and once he got it so low as 39°·6. The deepest lakes on the whole gave the lowest temperatures, but by no means always in exact proportion. In the lake of Geneva the bottom temperature at 950 feet was 41°·7; and in that of Lake Constance it was 39°·6, at 370 feet only. He thought the time of the year made little difference; but he did not try the same lake in the same place in different months. He tries to show that locality did not much affect the question of temperature. But this is surely a mistake; for the vicinity of snow-clad mountains, and the hard winter they occasion, are the probable causes why the cold at the bottom of the deep lakes there is greater than is observed in so much higher a latitude as Scotland.

In fact, the deep temperature of a very deep lake must be ruled far more by the cold of winter than by the heat of summer. The cold water must continue to descend as long as the cold months last. The colder these months are, the longer that cold lasts, the greater must be the cold at the bottom, and the thicker the stratum of cold water. The warmth of the air in summer and autumn acting only on the water by conduction, cannot move the deep cold substratum upwards. The only other heating influence from above, a far more penetrating influence, is the sun's rays.

But the water of Loch Lomond is scarcely transparent enough to allow the sun's heating rays to penetrate so deep as 500 or 600 feet, and the transparency of the lake of Geneva is not so much greater as to permit us to assume that the heating portion of the sun's rays can penetrate to 620 and 950 feet. It may be, nevertheless, that a slight effect may be produced even at these great depths in this way.

But there is still another heating power available for raising the cold substratum of water, and that is the heat of the earth at the bottom. At Loch Lomond, at 600 feet, this ought to be about 60°. At the bottom of the lake of Geneva it ought to be about 72°. It is true that the conducting power of the rocky exterior of the earth is too feeble to allow of much effect from this heating power, but it must have some influence, however small. In one way or another,—by heat from the sun's rays, or heat from the bosom of the earth, or by the joint action of both,—it may be that the cooling influence of the atmosphere will be to some little extent counteracted. If so, the amount of this counteracting effect will vary according to the severity or mildness of the winter months. In short, the bottom temperature will rise a little in autumn after a very open winter; and it will not stand so high after a very severe one.

We have had an excellent opportunity of testing this view during the past summer and autumn, on account of the uncommon deficiency of cold weather last winter,—so great a deficiency that, as stated in my communication last spring, the mean atmospheric temperature of the six cold months was at Loch Lomond, by the calculations of Mr Buchan, 1°·4 higher than the average for thirteen preceding years.

Has this circumstance had any effect on the bottom temperature of Loch Lomond in deep soundings?

On 10*th April*, as stated in the Proceedings of the Society, the temperature at 594 feet, as near as possible to the place of observation in September, October, and November last, was 42°,—exactly as in these months. On 6*th May*, much intervening sunshine having prevailed for nearly four weeks, but with a cold atmosphere, the surface-temperature at the same place had risen only from 43° to 44°·5; and the bottom temperature was 42°·1. I did not attach

any consequence whatever at the time to this difference. It might have been an error of observation; but three competent observers agreed in marking the index as at 42°·1. I had unhappily no opportunity of making any observation during the remaining summer months, which I now greatly regret. But on 8th August I went from Loch Goil to visit Dr Bennett at Loch Lomond, and with his assistance as an observer, got the following results, with the same thermometer as in former observations, viz., near the surface, 61°·5; at 200 feet, 44°; at 250 feet, 42°·6; at 300 feet, 42°·5; at the bottom, in 594 feet soundings, 42°·5. I returned on 22d August, and again, with Dr Bennett's check, obtained at the surface, 64°·5; at 300 feet, 42°·5; at 600 feet, 42°·4. A third time I returned on 19th September, and obtained at the surface, 57°·0; at 200 feet, 43·0°; at 582 feet, at the bottom, 42°·66.

Here then is an appreciable rise,—as to which I know not where a mistake can exist,—since the autumn of last year, and taking place during the warm months only.

It would be rash to draw deductions from the observations alone of two such autumns as those of 1871 and 1872, the one following a rather hard, the other an uncommonly open winter. But do not these observations establish some hope that a single good observation, made, let us say, in the middle of August, of September, and of October, may be found to denote the relative quality of our winters, and to mark out cycles of it?

Everything here depends on the fidelity of the observer and the accuracy of his instrument. On this account, and for the sake of those who, I trust, will repeat these observations from year to year, I have to remark that the thermometer I used was always the same, a protected thermometer, by Casella, instrument-maker to the Admiralty; that its scale at 60° and 40° agreed exactly with three others intended by their respective makers to be exact, one of them, indeed, made by Casella himself; and that I had an opportunity of ascertaining two days ago, that it is proof against pressure, in an excellent machine, constructed for Professor Wyville Thomson's expedition by the able engineer Mr Milne. Marking 55°·0 in the air, it came out after being exposed to a pressure equivalent to that of 3000 feet of water, marking 66° by the mercury in both limbs; and in the minimum side the index

remained exactly at 55°, while the index in the maximum side stood in close contact with the mercury at 66°. This instrument is subject to an alternative inconvenience, requiring nice adjustment of the force of the spring attached to the indices. If they are too tight, they may stick beyond the force of the magnet to move them, or so that the mercury may pass instead of pushing them. If they are too loose, a slight shock may alter their position. To avoid this risk, the simplest precaution is to paint the last eighteen feet of the line white. As the rest becomes deep brown in the water, the winder-up of the reel is at once apprised of the necessity of gradually slowing his speed before the instrument appears near the surface. The time necessary for the thermometer to assume a new temperature is considerable, and ought to be ascertained experimentally. Mine, instead of twelve hours, like that of De Saussure, takes seven minutes to move six degrees in a gentle current of uniform temperature. It had seldom to pass through so many degrees between one observation and another; but I allowed it always eight, and generally ten minutes, and in important observations near the bottom even fifteen or twenty minutes, for absolute security. But I believe ten minutes to be in all circumstances more than sufficient.

The late Mr James Jardine, civil engineer, and during his lifetime a prominent Fellow of this Society, made in 1812 and 1814 observations in Loch Lomond, Loch Katrine, and Loch Tay similar to De Saussure's and my own. These valuable observations have been recovered by Mr Leslie, in the form of the original draught, and have been communicated to the Society by Mr Buchan; but I find that most of them had appeared in Sir John Leslie's article *Climate*, in the "Encyclopædia Britannica," and again in an octavo collection of Sir John's treatises in that work, edited in 1838 by the late Principal Forbes. Jardine's observations may yet turn out more valuable than he could have anticipated, and already seem to me of such interest as to deserve further notice.

His experiments were made early in September. In Loch Lomond, in 1812 he found near the surface a temperature of 59°·3; at 240 feet, 41°·3; at the bottom, in 600 feet soundings, 41·1°. On Loch Katrine, the day previous, he found 57°·3 near the surface;

at 210 feet, 41°·1; at 480 feet, close to the bottom, 41°·0 Again, on Loch Katrine in 1814, four days earlier than in 1812, he found near the surface, 56°·4; at 180 feet, 41°·9; at the bottom, 41°·3. On Loch Tay, in August 1812, he found at the surface, 57°·2; at 210 feet, 43°·2; at the bottom, 420 feet, 41°·9. These results, if we could only know exactly how they were obtained, are singularly interesting as comparative with mine, got about sixty years afterwards. If they be quite accurate, they indicate a bottom-temperature decidedly below what I have always obtained; and this is quite intelligible under the view I have taken of the probability of annual change, according to the character of the preceding winter; for all the winters preceding the times of Mr Jardine's observations were uncommonly severe. Or, taking a different view of the facts, these comparative observations give no countenance to the fanciful announcement by some late meteorological alarmists, that the climate of Great Britain is undergoing progressive deterioration by descent of the polar ice. Accurate deep-water observations in our deep lakes will in time very easily test this hypothesis; if Jardine's and my own be both correct, they may denote certainly no deterioration, but, if any change, a slight improvement rather. But I have shown how the difference probably arose from a temporary peculiarity of the climate of each year observed. As to Mr Jardine's observations, we cannot now learn exactly how he worked, and we can trust for their correctness only to the character, universally allowed him during his life, of being a singularly acute, exact, conscientious observer of all physical facts and phenomena.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

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NINETIETH SESSION.

Monday, 16th December 1872.

SIR ALEXANDER GRANT, Bart., Vice-President, in the Chair.

The following Communications were read:—

1. On the Philological Genius and Character of the Neo-Hellenic Dialect. By Professor Blackie.

The Author showed by a historical review of the fortunes of Greece, through the Middle Ages, and under the successive influences of Turkish conquest and Turkish oppression, how the Greek language had escaped corruption to the degree that would have caused the birth of a new language in the way that Italian and the other Roman languages grew out of Latin. He then analysed the modern language, as it existed in current popular literature before the time of Coraes, that is, from the time of Theodore Ptochoprodromus to nearly the end of the last century, and showed that the losses and curtailments which it had unquestionably suffered in the course of so many centuries, were not such as materially to impair the strength and beauty of the language, which in its present state was partly to be regarded as a living bridge betwixt the present and the past, and as an altogether unique phenomenon in the history of human speech.

2. Laboratory Notes. By Professor Tait. Communicated, in his absence, by Professor G. Forbes.

1. On the Relation between Thermal, and Electric, Conductivity.

Reference was made to a previous paper by the author (Proceedings, 1867–8, p. 309), in which an attempt was made to apply to this subject the Theory of Dissipation of Energy.

Some years ago, a bar of German silver was procured, at the expense of the British Association, for the purpose of ascertaining whether its thermal, like its electric, conductivity is little altered by change of temperature. With this, Forbes' experiments have been carried out carefully through very great ranges of temperatures.

The exceedingly laborious calculations necessary to a complete determination are not yet carried out; but, by a rough graphic method, it has been ascertained that the alteration of conductivity, by rise of temperature, is at least very small compared with that observed in iron under the same circumstances.

2. On Electric Conductivity at a Red Heat.

This was a mere preliminary notice of what promises to be at once an interesting and an extended inquiry, to which I have been led by some recent results in thermo-electricity. At present, it may be stated that at, and above, a red heat the electric conductivity of iron seems to fall off much faster with increasing temperature than that of platinum. To such an extent does this take place, that I have endeavoured (as yet, however, unsuccessfully) to form a circuit in which the main resistance is an iron wire, and to obtain a *maximum* current by gradually shortening the wire. The fall in conducting power seems so very marked that some of it will remain, even I believe when allowance is made for the oxidation of the iron. I have ordered a special apparatus for the purpose of avoiding this source of uncertainty.

3. On the Thermo-Electric Relations of Pure Iron.

By the kindness of Dr Russell, of Bartholomew's Hospital, I have been enabled to experiment upon a ribbon of pure iron prepared by

the late Dr Matthiessen. I reserve details until I can obtain the history and mode of preparation of the specimen examined, but I may state now that, when formed into thermo-electric circuits with various alloys of Platinum and Iridium (Proceedings, 1871–2, p. 773) it gives results, as to the position of neutral points, not differing more from those given under the same circumstances by various iron wires of commerce, than the latter do among themselves. Thus it appears that, in the thermo-electric diagram, the line even for pure iron is sinuous; and that the specific heat of electricity in it changes sign somewhere about a low red heat.

3. Note on the Rate of Decrease of Electric Conductivity with Increase of Temperature. By D. H. Marshall, M.A., Assistant to the Professor of Natural Philosophy. Communicated by Professor Tait.

These experiments were undertaken in order to determine how closely the hypothesis "that the electric resistance in a pure metal is directly as its absolute temperature" holds for various metals at two easily ascertained temperatures,—that of the air in the room, and the boiling point of water. The apparatus used was a Wheatstone's bridge; one coil of wire kept in a vessel of water at the temperature of the air in the room being put against another, which could be heated up to 100° C. The experiments showed that the rate of increase of resistance with temperature was different for hard and soft specimens of the same metal, being always less in the hard. This was further proved by additional experiments, which showed that sudden cooling always diminished the rate of increase of resistance, whereas if the metal were allowed to cool slowly after being boiled, the rate of increase of resistance was always sensibly increased.

The first two columns of figures give the ratio of the resistances at the two temperatures; the first and third give the ratios of the temperatures themselves in absolute scale; the fourth is the difference between the second and third, which will therefore show the amount and direction of deviation from the above hypothesis. When the number in the fourth column is +, the rate of increase

of resistance with temperature is less than it would be according to the hypothesis; when –, greater.

Soft Crown Cu.	374	280	284·4	– 4·4
"	"	283	284·1	– 1·1
"	"	281	283·6	– 2·6
Soft C. Cu.	374	288·6	287	+ 1·6
"	"	285·6	285	+ ·6
"	"	286·7	285·8	+ ·9
Hard C. Cu.	374	319	288·1	+ 30·9
"	"	321	288·5	+ 32·5
"	"	319·6	288	+ 31·6
Soft Pt.	374	303·9	288·8	+ 15·1
"	"	303·9	289·2	+ 14·7
Hard Pt.	374	356	288·2	+ 67·8
Cd.	374	291·5	295·7	– 4·2
"	"	285	292·9	– 7·9
"	"	286·2	293·3	– 7·1
Au.	374	301·7	292·7	+ 9·0
"	"	304	292·2	+ 11·8
"	"	302·4	293·1	+ 9·3
Ag.	374	302·3	291·5	+ 10·8
"	"	303·1	291·8	+ 11·3
"	"	304·7	292·9	+ 11·8
Zn.	374	294·1	294·1	0·0
"	"	290·3	294·2	– 3·9
"	"	288·6	293·6	– 5·0
Fe.	374	283·8	292·8	– 9·0

Monday, 6th January 1873.

DAVID MILNE HOME, LL.D., Vice-President, in the Chair.

The CHAIRMAN said:—Before the papers in our to-night's programme of business are taken up there is a statement which, at the special request of the Council of this Society, I have to make from the chair. I have been requested to allude to the deaths of three much esteemed Fellows of this Society, which have occurred since

our last ordinary meeting—Professor Macquorn Rankine of Glasgow University; the Very Rev. Dean Ramsay, Edinburgh; and Archibald Smith of Jordanhill. The Council think that it is only a fitting tribute to the memory of our deceased colleagues that I should express regret at the loss which we, in common with others, have sustained, and that I should also briefly allude to their connection with this Society and with science, leaving to a future occasion the duty of giving a fuller biographical account of each. Professor Rankine, when he died, was one of our vice-presidents, having joined the Society in the year 1850. Important scientific investigations were carried on by him, and were the subjects of numerous papers read at our meetings, and published in our Transactions. A series of six papers "On the Mechanical Action of Heat" gained for him our Keith prize in the year 1853. Our Transactions also contain papers by him "On the Centrifugal Theory of Electricity," "On the Specific Heat of Water at various Temperatures," "On the Absolute Zero of the Gas Thermometer," and "On the Thermal Efficacy of Molecular Vortices." Professor Rankine was not only the most eminent Professor of Engineering known in Great Britain, but he was also distinguished for his knowledge of pure science. His merit as a man of science was recognised by the British Association when he was chosen to be president, once of their mathematical, and twice of their mechanical sections. Glasgow University has by the death of this eminent man lost one of her most useful professors, so that in many quarters the utmost regret will be felt at his death. The Very Rev. Dean Edward Bannerman Ramsay was an M.A., an LL.D., and Fellow of our Society. In the years 1828 and 1829 he was one of the secretaries of our ordinary meetings, and in the years 1859 to 1861, he was one of our vice-presidents. In the year last named, at the special request of the council, he gave an opening address from this chair on the commencement of the winter session, which address was published in our Proceedings. The only paper read by him to the Society on a particular subject was a biographical memoir of the Rev. Dr Chalmers, with whom he had been on terms of friendship; and the memoir was published in our Transactions. I may add, that one of the last public acts of the Dean, other than professional, was to convene a meeting in this city, to obtain funds for a monu-

ment to Dr Chalmers, and this movement, I may add, proved so successful, that as the result of it, a colossal statue of that eminent man is now being executed, and will soon, I hope, adorn one of the principal streets of our city. This is neither the occasion nor the place for referring to Dean Ramsay's usefulness and reputation as a divine, or as a pastor of a large and attached congregation. Neither can I do more here than allude to the many excellent discourses and treatises on religious subjects, of which he was the author. But I cannot forbear mentioning, and with special emphasis, the Dean's geniality of disposition, his large-heartedness, and his entire freedom from sectarian jealousy, which enabled and disposed him to acccept, and even to seek, the society and friendship of any person of worth, though not belonging to his own branch of the Christian Church. May I be permitted to express a wish and entertain a hope that the example he set, approved of as it is by, I believe, all classes of this community, may not be without good effect. Dean Ramsay, though faithful and assiduous in the performance of his professional duties, found time for acquiring information and pursuing studies in other fields. He was extremely fond of music, and his knowledge of it, even in its scientific aspects, was well shown in two lectures which he delivered before the Philosophical Institution of the city "On the Genius and Works of Handel." His knowledge of botany was shown in a memoir which he published of the discoveries and works of his friend Sir J. E. Smith. But the literary work which carried his name farthest, and will preserve it longest, at least among his countrymen, was his "Reminiscences of Scottish Life and Character." It is a striking proof of the general appreciation of this book, that it went through twenty editions, and that only a fortnight before his death, the venerable Dean was revising the proof sheets of a twenty-first edition. Dean Edward Ramsay was a Scotchman of whom his country has reason to be proud, and who will live in the hearts of all who had the happiness to possess his personal acquaintance. Archibald Smith, of Jordanhill, was an LL.D. and F.R.S. of London and Edinburgh. He first distinguished himself as a student of Glasgow University, and afterwards in Cambridge, having, at Trinity College there, acquired the high position of Senior Wrangler and first Smith's prizeman. Though he became by profession an English barrister,

his tastes were for mathematics and physics. He was employed by Government to make a reduction of important magnetic observations carried on by two Government ships in the Antarctic regions. He was employed at the suggestion of Sir Edward Sabine and Professor Airy, both of whom were well acquainted with his mathematical powers. The chief value of his services lay in his correcting the effect on the magnetic observations due to the iron in the ships. He afterwards, under the sanction of Government, drew up and published an "Admiralty Manual for the Deviation of the Compass," a work greatly appreciated, and which has been republished in various languages. Mr Smith received from the Royal Society of London one of its Royal Medals; from the Emperor of Russia, a beautiful compass set with diamonds; and from our own Government, a gift of L.2000, in acknowledgment of his important scientific services. The three individuals to whom I have now referred were each, in their different vocations, distinguished by high character, superior talents, and useful lives; and I am sure that all present will approve of the tribute of respect to their memory, which in name of the Council I have now, however imperfectly, attempted to offer.

The following Communications were read :—

1. On a Question of Arrangement and Probabilities. By Professor Tait.

Many of the common illustrations of probabilities are taken from games in which each hand, or trick, *must* necessarily be won by one player, and lost by the other. It becomes an interesting question to inquire what modification is introduced if we contemplate the possibility of a hand, or trick, being drawn—*i.e.* not won or lost by either player. The only difficulty lies in taking account of the limiting conditions.

In the game of golf, for instance, where each hole separately may be won, halved, or lost, we have the following question. When a player is x holes "up," and y "to play," in how many ways may he win?

Let this number be represented by $P_{x,y}$. Then obviously

$$P_{x+1,y+1} = P_{x+2,y} + P_{x+1,y} + P_{x,y}.$$

If

$$P_{x,y} = a^x b^y$$

be a particular integral, we have

$$ab = a^2 + a + 1,$$

so that

$$P_{x,y} = \Sigma C a^x \left(a + 1 + \frac{1}{a}\right)^y.$$

Now the conditions are obviously

$$P_{x,y} = 1, \text{ if } x > y;$$

and

$$P_{-x,y} = 0, \text{ if } x \overline{\overline{>}} y.$$

Failing in several attempts to determine fully the special form of $P_{x,y}$ from these conditions, I had recourse to a graphical method, which will be given below. But before I do so, I take another mode of integration, which leads easily to special numerical results.

Suppose $y = x + n$,
then the equation becomes

$$\Delta P_{x,x+n} = P_{x+2,x+n} + P_{x+1,x+n}$$

from which it appears that if we can find expressions for $P_{x,x+m}$ and $P_{x+1,x+m}$ we can deduce by summation that for $P_{x-1,x+m}$.

Let us first put $n = 0$; we have

$$\Delta P_{x,x} = P_{x+2,x} + P_{x+1,x} = 2,$$

since, obviously, each of these quantities is unity. Integrating, we have

$$P_{x,x} = 2x,$$

no constant being added, since it is clear that

$$P_{0,0} = 0.$$

Again, by the fundamental equation, putting $n = 1$, we have

$$\begin{aligned} \Delta P_{x,x+1} &= P_{x+2,x+1} + P_{x+1,x+1} \\ &= 1 + 2(x+1) \\ P_{x,x+1} &= x + x(x+1) + C \\ &= (x+1)^2 = x(x+1) + (x+1) \end{aligned}$$

for we have obviously

$$P_{0,1} = 1 .$$

Next,

$$\Delta P_{x,x+2} = P_{x+2,x+2} + P_{x+1,x+2}$$

$$= 2(x+2) + (x+2) + (x+1)(x+2) .$$

$$P_{x,x+2} = \frac{3}{2}(x+1)(x+2) + \frac{1}{3}x(x+1)(x+2) ,$$

no constant being added, for

$$P_{0,2} = 3 .$$

Similarly,

$$P_{x,x+3} = \frac{5}{6}(x+1)(x+2)(x+3) + \frac{1}{2}(x+2)(x+3) + \frac{1}{12}x(x+1)(x+2)(x+3),$$

for

$$P_{0,3} = P_{1,2} + P_{0,2} + P_{-1,2} = 4 + 3 + 1 = 8 .$$

$$P_{x,x+4} = \frac{2}{3}(x+2)(x+3)(x+4) + \frac{7}{24}(x+1)(x+2)(x+3)(x+4) + \frac{1}{60}x(x+1)(x+2)(x+3)(x+4)$$

for

$$P_{0,4} = P_{1,3} + P_{0,3} + P_{-1,3} = 11 + 8 + 4 = 23.$$

We may now, in conformity with these expressions, assume

$$P_{x,x+n} = \left\{ A_n + \frac{B_n}{x} + \frac{C_n}{x(x+1)} + \ldots \right\} x\,\overline{x+1} \ldots \overline{x+n}$$

Now, if $y = x + n$, the original equation of differences gives

$$\Delta P_{x,x+n} = P_{x+2,x+n} + P_{x+1,x+n}$$

where Δ refers to x and *not* to n. By the assumed value of $P_{x,x+n}$ this becomes

$$\left[\frac{(n+1)A_n}{x} + \frac{nB_n}{x(x+1)} + \frac{(n-1)C_n}{x(x+1)(x+2)} + \cdots \right] x\,\overline{x+1} \cdot \cdot \overline{x+n}$$

$$= \left[A_{n-2} + \frac{B_{n-2}}{x+2} + \frac{C_{n-2}}{(x+2)(x+3)} + \cdots \right] \overline{x+2} \cdot \cdot \overline{x+n}$$

$$+ \left[A_{n-1} + \frac{B_{n-1}}{x+1} + \frac{C_{n-1}}{(x+1)(x+2)} + \cdots \right] \overline{x+1} \cdot \cdot \overline{x+n}$$

Whence, equating coefficients of like factorials, we have

$$(n+1)\,A_n = A_{n-1}\,,$$
$$nB_n = B_{n-1} + A_{n-2}\,,$$
$$(n-1)\,C_n = C_{n-1} + B_{n-2}\,,$$
$$(n-2)\,D_n = D_{n-1} + C_{n-2},\ \text{\&c., \&c.}$$

Let

$$\underline{|n+1}\,A_n = \alpha_n,\quad \underline{|n}\,B_n = \beta_n\,,\quad \underline{|n-1}\,C_n = \gamma_n,\ \text{\&c.}$$

then these equations become

$$\begin{array}{r|c|l}
\alpha_n = \alpha_{n-1} & & \alpha_n = \Sigma 0. \\
\beta_{n+1} = \beta_n + \alpha_{n-1} & & \beta_n = \Sigma\alpha_{n-1} = \frac{\Sigma}{D}\alpha_n \\
 & \text{or} & \\
\gamma_{n+1} = \gamma_n + \beta_{n-1} & & \gamma_n = \Sigma\beta_{n-1} = \frac{\Sigma}{D}\beta_n \\
\delta_{n+1} = \delta_n + \gamma_{n-1} & & \delta_n = \Sigma\gamma_{n-1} = \frac{\Sigma}{D}\gamma_n,\ \text{\&c.}
\end{array}$$

Thus we have

$$P_{x,\,x+n} = \left\{ \frac{\alpha_n}{\underline{|n+1}} + \frac{\beta_n}{x\,\underline{|n}}\frac{\Sigma}{D}\alpha^n + \frac{1}{x(x+1)\,\underline{|n-1}}\left(\frac{\Sigma}{D}\right)^2\alpha^n + \ldots \right\} x(x+1)\ldots(x+n)$$

$$= \left\{ \alpha_n + \frac{n+1}{x}\frac{\Sigma}{D}\alpha_n + \frac{n+1\,n}{x(x+1)}\left(\frac{\Sigma}{D}\right)^2\alpha_n + \ \ldots \right\} \frac{x(x+1)\ .(x+n)}{1.2\ \ldots(n+1)}$$

$$= \frac{1}{\left(\frac{\Sigma}{D}\right)^{x-1}} \left\{ \frac{\overline{x+n}\ldots\overline{n+2}}{1.2\ \ldots\overline{n-1}}\left(\frac{\Sigma}{D}\right)^{x-1} + \frac{\overline{x+n}\ldots\overline{n+1}}{1.2\ \ldots\ n}\left(\frac{\Sigma}{D}\right)^{x} + \ldots \right\}\alpha_n$$

$$= \frac{\left(1+\frac{\Sigma}{D}\right)^{x+n}}{\left(\frac{\Sigma}{D}\right)^{x-1}}\,\alpha_n\,,$$

for no negative powers of $\frac{\Sigma}{D}$ are to be retained, as α_n is a mere constant.

The trouble of carrying out this process is considerable, depend-

ing on the determination of the constants in each finite integral so as to satisfy the limiting conditions of the problem. To a few terms we have

$$P_{x,x+n} = \frac{|\underline{x+n}}{|\underline{x-1}\;|\underline{n+1}}\left\{2 + (2n-1)\frac{n+1}{x} + (n-2)^2\frac{(n+1)n}{x(x+1)} + \ldots\right\}.$$

By a slight modification of the preceding process we get in succession

$$\Delta P_{-x,\,x+n} = P_{-(x+1),\,x+n} + P_{-(x+2),\,x+n}\,,$$

$$_{x,\,x+n} = \frac{\left(1+\frac{\Sigma}{D}\right)^{x+n}}{\left(\frac{\Sigma}{D}\right)^{x+1}}\,a_n = \frac{|\underline{x+n}}{|\underline{x+1}\;|\underline{n-1}}\left\{1 + (n-1)\frac{n-1}{x+2} + \frac{(n-4)(x-1)}{2}\frac{(n-2)(x-1)}{(x+2)(x+3)} + \ldots\right\}.$$

The graphical method to which I referred above consists simply in supposing the various values of $P_{x,y}$ to be written each at the point whose co-ordinates are the values of x and y. If, to fix the ideas, we suppose the axis of x to be horizontal and that of y vertically downwards, then the fundamental equation shows that *by adding together any three contiguous numbers in a horizontal line, we produce the number immediately under the middle one of the three.*

The limiting conditions show that all the numbers along the line

$$x + y = 0\,,$$

and those between it and the negative part of the axis of x, are zeros; while those along

$$y = x - 1\,,\; y = x - 2\,,\; y + x = 1\,,$$

are each equal to 1.

Hence we have the figure

0	0	0	0	0	0	1	1	0	0	0	0	 x
0	0	0	0	0	1	2	1	1	0	0	0	
0	0	0	0	1	3	4	4	1	1	0	0	
0	0	0	1	4	8	11	9	6	1	1	0	. . (a)
0	0	1	5	13	23	28	26	16	8	1	1	
0	1	6	19	41	64	77	70	50	25	10	1	
					⋮							
		&c.			y			&c.				

where the numbers printed in darker type are inserted by the rule given above. This is, of course, in one sense a complete solution of the problem; but the results may easily be put in an analytical form.

Had we had zeros along the line

$$y = x - 2$$

we should have had the following scheme instead of that above:

$$\begin{array}{cccccccccccc}
 & & & & 0 & 1 & 0 & & \cdot & & \cdot & x \\
 & & & 0 & 1 & 1 & 1 & 0 & & & & \\
 & & 0 & 1 & 2 & 3 & 2 & 1 & 0 & & \cdot & \cdot \\
 & 0 & 1 & 3 & 6 & 7 & 6 & 3 & 1 & 0 & & \\
0 & 1 & 4 & 10 & 16 & 19 & 16 & 10 & 4 & 1 & 0 & \\
 & & & & | & & & & & & & \\
 & \&c. & & & y & & & \&c. & & & &
\end{array} \qquad (b)$$

Hence the part added by the units along the line

$$y = x - 2$$

is

$$\begin{array}{ccccccccc}
 & & & 0 & & \cdot & & \cdot & x \\
 & & & 0 & 1 & & & & \\
 & & 0 & 1 & 1 & 2 & & & \\
 & 0 & 1 & 2 & 4 & 3 & 3 & \cdot & \cdot \\
0 & 1 & 3 & 7 & 9 & 10 & 6 & 4 & \\
 & & & | & & & & & \\
 & & & y & & \&c. & & &
\end{array} \qquad (c)$$

This, again, differs from (*b*) shifted one place downwards, by

$$\begin{array}{cccccc}
0 & & \cdot & & \cdot & x \\
0 & 0 & & & & \\
0 & 0 & 1 & & & \\
0 & 1 & 1 & 2 & \cdot & \cdot \\
1 & 2 & 4 & 3 & 3 & \\
y & & \&c. & & &
\end{array} \qquad (d)$$

But it is obvious that this is a repetition of the same one place diagonally downwards to the right.

Also (b) is obviously the coefficients of the powers of a in

$$a\left(a+1+\frac{1}{a}\right)^y$$

for the several positive integral values of y. Call the term in a^x in this, *i.e.*, the coefficient of a^{x-1} in $\left(a+1+\frac{1}{a}\right)^y$, $A_{x,y}$, and that at x, y in the scheme (c) $Q_{x,y}$, then

$$Q_{x,y} - Q_{x-1,\,y-1} = A_{x,y-1}\ .$$

and thus

$$\begin{aligned} P_{x,\,y} &= A_{x,\,y} + Q_{x,\,y} \\ &= A_{x,\,y} + A_{x,\,y-1} + A_{x-1,\,y-2} + \ .\ \ .\ \ .\ \ .\ \ .\ \ . \end{aligned}$$

This points to a very simple way of constructing the values of $P_{x,\,y}$ from those of $A_{x,\,y}$.

In scheme (b), add to the number in any position that immediately above it, and also those lying in the left handed upward diagonal drawn from the last named, their sum is the number in the corresponding position in (a). Thus $16+6+3+1=26$.

If D refer to x and D′ to y, we have

$$P_{x,\,y} - \left(1+\frac{1}{D'}+\frac{1}{DD'^2}+\frac{1}{D^2D'^3}+\ .\ .\ .\right)A_{x,\,y},$$

$$=\left(1+\frac{D}{DD'-1}\right)A_{x,\,y}.$$

It is to be observed that, since if one player wins the other must lose, $P_{-x,\,y}$ is the number of ways in which a player may lose, when he is x "up" and y "to play."

The number of ways in which the game may be drawn is also a solution of the same equation of differences; but the limiting conditions are now obviously independent of the sign of x: and are, taking it positive,

$$P_{x,\,y} = 1 \text{ if } x = y\,,$$
$$P_{x,\,y} = 0 \text{ if } x > y\,.$$

Hence the values are represented by the following scheme—

$$\begin{array}{ccccccccc} & & 0 & 1 & 0 & . & . & . & . \quad x \\ & & 0 & 1 & 1 & 1 & 0 & & \\ & 0 & 1 & 2 & 3 & 2 & 1 & 0 & \\ 0 & 1 & 3 & 6 & 7 & 6 & 3 & 1 & 0 \\ & & \&c. & & y & & \&c. & & \end{array}$$

Thus the value of $P_{x,y}$ in this case is the coefficient of a^x in

$$\left(a + 1 + \frac{1}{a}\right)^y.$$

Hence the number of different modes in which the game may finish, when one of the players is x "up," and there remains y "to play" is, calling $R_{x,y}$ the coefficient of a^x in $\left(a + 1 + \frac{1}{a}\right)^y$,

$$\left[\left(D + \frac{1}{D}\right)\left(1 + \frac{D}{DD'-1}\right) + 1\right]R_{x,y},$$

while the number of different ways of finishing if the whole y holes are played out is 3^y.

There are many very curious properties of the numbers we have denoted by $P_{x,y}$, $A_{x,y}$, $Q_{x,y}$. Thus, for instance, it is easy to see that

$$\begin{array}{ll} Q_{1,2} = Q_{2,2} - 1 & Q_{0,2} = Q_{3,2} + 1 \\ Q_{1,3} = Q_{2,3} + 1 & Q_{0,3} = Q_{3,3} - 1 \end{array} \quad \&c.$$

all of which are included in

$$Q_{x,y} = Q_{3-x,y} + (-1)^{x+y}.$$

2. Laboratory Notes. By Professor Tait.

1. On the Stiffness of Wires.

The following are the results of some experiments made for me by Mr W. M. Ogilvie with Amontons' apparatus; chiefly with the view of testing the accuracy with which it can be applied, but incidentally with the view of obtaining an idea of the relation between tension and stiffness in the same wire or cord.

(*a*) Fine Iron Wire.

Weight on each end of Wire.	Weight required to overcome stiffness of Wire.	
4,000 grains	170 grains	
5,000 „	200 „	
7,000 „	230 „	Roller used is $1\frac{1}{2}$ in. diameter.
12,000 „	270 „	
15,000 „	280 „	
22,000 „	300 „	

(*b*) Fine Copper Wire, annealed over a gas flame.

1000 grains	27 grains
2000 „	40 „
3000 „	50 „
4000 „	56 „
5000 „	60 „
8000 „	64 „

The results which immediately follow were obtained from annealed and unannealed wires, of the same gauge, of two very different kinds of copper—crown being of very high, C of very low, thermal and electric conductivity.

(*c*) Soft C Wire.

5,100 grains	1240 grains
9,100 „	1300 „
22,100 „	1370 „
40,100 „	1400 „

(*d*) Soft Crown Wire.

5,100 grains	1340 grains
9,100 „	1400 „
22,100 „	1500 „
40,100 „	1560 „

(*e*) Hard C Wire.

5,100 grains	1400 grains
12,100 „	1540 „
22,100 „	1740 „
41,100 „	1900 „

(*f*) Hard Crown Wire.

Weight on each end of Wire.	Weight required to overcome stiffness of Wire.
5,100 grains	1500 grains
9,100 "	1600 "
22,100 "	1800 "
40,100 "	2000 "

(*g*) Same Wire as (*e*), after several experiments with it.

10,000 grains	1340 grains
30,000 "	1600 "
50,000 "	1800 "
60,000 "	1900 "

(*h*) Soft Wire, another specimen of (*d*), after a good number of trials, and taking the average of the last three.

10,000 grains	1000 grains
30,000 "	1190 "
50,000 "	1360 "
60,000 "	1460 "

(*k*) Same Wire as last *doubled.*

10,000 grains	2300 grains	No great precautions were taken in this experiment to secure the weight being equally distributed over both wires.
30,000 "	2470 "	
50,000 "	2640 "	
60,000 "	2740 "	

No proper results could be got by *doubling* any of the unannealed wires.

(*l*) Whip Cord.

10,000 grains	130 grains
20,000 "	240 "
30,000 "	400 "
40,000 "	530 "
50,000 "	630 "
60,000 "	700 "

With one exception these results indicate a logarithmic relation between the stiffness (S) and the tension (T) of the form

$$S_0 - S \propto a^{-T}.$$

Here S_0 is the stiffness when the tension is very great.

Thus, taking the numbers in experiments (*b*) and (*l*) above, we obtain the following comparison with experiment of the formulæ—

$$68 - S = 61{\cdot}5 \times \left(\tfrac{2}{3}\right)^{\frac{T}{1000}}. \qquad 1950 - S = 1987 \times (1{\cdot}08)^{-\frac{T}{10,000}}$$

Experiment.	Formula.	Experiment.	Formula.
41	41	182	184
28	27·3	171	170
18	18·2	155	158
12	12·1	142	146
8	8	132	135
4	2·4	125	125

Considering the excessively uncertain nature of such experiments, these results may be looked upon as agreeing well with the law suggested.

2. Preliminary Sketch of the Thermo-electric Diagram for Iron, Gold, and Palladium.

3. On the Muscles which open and close the Mouth, with some Observations on the Active and Passive Condition of Muscles generally. By Dr Gamgee.

4. Observations and Experiments on the Cerebral Hemispheres and Corpora Striata of Birds. By Dr M'Kendrick. Communicated by Professor Turner.

Surgeon-Major Black exhibited twenty-five large photographic views of the late eruption of Mount Vesuvius, which had been executed by his brother, John Melton Black, Esq.

Monday, 20th January 1873.

Sir WILLIAM STIRLING MAXWELL, Bart., Vice-President, in the Chair.

The Council of the Royal Society have awarded the Makdougall Brisbane Prize to George James Allman, M.D., F.R.S., Emeritus Professor of Natural History in the University of Edinburgh, for his memoir "On the Homological Relations of the Cœlenterata," published in the Transactions of the Society for 1870–71.

In selecting this memoir for the prize, they took into consideration not merely its own importance as a contribution to zoological science, but the author's elaborate and beautifully illustrated monograph "On the Gymnoblastic or Tubularian Hydroids," published in two large folio volumes by the Ray Society, of which it forms a leading chapter.

This monograph comprises a most extensive series of researches into the morphology, development, minute structure, and physiology of an interesting group of invertebrated animals, as well as a careful consideration of their zoological position and classification.

It contains the observations and conclusions of many years of laborious research, and whilst serving as a memorial of the industry, artistic skill, and scientific acumen of its author, forms a most important contribution to natural history science.

The following Report was submitted to the Society:—

Report by the Council of the Royal Society of Edinburgh on the proposed alterations of the laws as to the Election of Ordinary Fellows:—

The Council have carefully considered the proposed alterations in the laws as to the election of Ordinary Fellows which were remitted to them by the Society for re-consideration on 3d June last, and have agreed to recommend to the Society that there should be no limitation in the number of Fellows annually elected, and that the following alterations should be adopted:—

That Laws IX. and XIII. should be altered as follows:—

IX.

Candidates for admission as Ordinary Fellows shall make an application in writing, and shall produce along with it a certificate of recommendation to the purport below,* signed by at least *four* Ordinary Fellows, two of whom shall certify their recommendation from personal knowledge. This recommendation shall be delivered to the Secretary, and by him laid before the Council, and shall afterwards be printed in the circulars for three Ordinary Meetings of the Society, previous to the day of election, and shall lie upon the table during that time.

XIII.

The election of Ordinary Fellows shall only take place at the first Ordinary Meeting of each month during the Session. The election shall be by ballot, and shall be determined by a majority of at least two-thirds of the votes, provided twenty-four Fellows be present and vote.

The Society adopted these alterations of the Laws.

The following Communications were read:—

1. On the Physical Constants of Hydrogenium. I. By Mr James Dewar.

2. On the supposed Upheaval of Scotland, in its Central Parts, since the time of the Roman Occupation By D. Milne Home, LL.D.

No abstract of this paper is given in the Proceedings, as the paper will appear in the Transactions.

* "A. B., a gentleman well-skilled in Science (*or Polite Literature, as the case may be*), being to our knowledge desirous of becoming a Fellow of the Royal Society of Edinburgh, we hereby recommend him as deserving of that honour, and as likely to prove a useful and valuable Member."

Monday, 3d February 1873.

SIR ROBERT CHRISTISON, Bart., President, in the Chair.

The following Communications were read :—

1. On the Anatomy of a new Species of Polyodon, the *Polyodon gladius* of Martens, taken from the river Yang-tsze-Kiang, 450 miles above Woosung. Part I., being its *External Characters and Structure.* By P. D. Handyside, M.D.

The position of this new species of Ganoid, under our commonly accepted classification, the author gave as follows :—

Division	Vertebrata.
1st primary section, . .	Ichthyopsida.
1st class, ...	Pisces.
3d sub-class, ...	Ganoidei.
2d order, ...	Chondrostei.
1st family, ...	Acipenseridæ.
2d family, ...	Polyodontidæ.
Genus, ...	Polyodon.
1st species, ...	P. folium.
2d species, ...	P. gladius.

After referring to the Polyodon *folium* of Lacépède (the *P. reticulata* of Shaw, the *Planirostra spatula* of Owen), the paddle-fish or spoon-bill sturgeon of the Ohio and Mississippi and their tributaries, as a well-known species of the genus in question, Dr Handyside went on to state that the new species now to be described was first observed on a Chinese fishmonger's stall at Woosung, 12 miles from Shanghai, and had since been found in the Yang-tsze-Kiang, and, as was alleged, in the northern Japanese sea. He then sketched the history of the Polyodontidæ family, and narrated the researches of Lacépède, Von Martens, Blakiston, Kaup, and Duméril.

He next exhibited to the Society—*first*, a small entire specimen

of the *P. gladius*, measuring 26½ inches = 652 millimeters; *secondly*, an opened specimen measuring 40 inches = 1070 millimetres; *thirdly*, three pieces of an adult fish that measured fully 9 feet long = 2720 millimetres; and *fourthly*, a piece of an almost adult fish that was not measured. He showed also four large drawings, and twenty-four smaller ones (including nineteen microscopic views), illustrative of his description of the External Characters and Structure of the fish, under the ten following heads:—

1st, Its size, weight, &c.; 2d, its form; 3d, its surface and colour; 4th, its fins; 5th, its proportional parts; 6th, its lateral line and system of muciparous pores; 7th, its exo- or dermo-skeleton and tegumentary system; 8th, its spatula, rostrum, or snout; 9th, its eyes, mouth, and teeth; and 10th, its branchiæ, pseudobranchiæ, and spiracula.

In the course of his paper, the author remarked that a specimen had been seen by Mr H. G. Hollingworth, resident at Kiu-Kiang, on the same river, reaching to the length of 15 feet, and weighing 133 lbs.; that in regard to edible properties, the young fish was said to be very delicate eating; that the body was compressed, elongated, and tapering towards the tail, like the sturgeon family generally. The head was projected beyond the mouth into an elongated muzzle or spatula. This snout was thin at the margins, but thick and keeled in the centre; in young specimens it was sharp at the point, but it afterwards got blunted and rounded off by digging among the silt of the river bottom. The eyes of the fish were of very small size, and it was supposed that the sensibility of the spatula compensated for the want of larger ones.

The 2d Part of Dr Handyside's paper will consist of an anatomical description of the nervous and muscular systems; the 3d Part of the viscera of organic life; and the 4th Part of the articular system and the endo-skeleton of the *Polyodon gladius*.

2. Note on the Thermal Equivalents of the Oxide of Chlorine. By James Dewar, Esq.

Two years ago the author submitted to the British Association a preliminary report on the subject, which has not been prosecuted,

owing to the exhaustive investigations of Professor Thomsen of Copenhagen, on thermal values. In the paper referred to, the results are calculated on the assumption of hydriodic acid evolving 15,000 heat units for equivalent in aqueous solution. The above number is much too high, according to Thomsen's recent experiments, who gave 13,170 as the true number. If my former results are recalculated with the new value for hydriodic acid, the following numbers are obtained:—

Formation of	Iodic acid in aqueous solution	=	25,000	heat units.
"	Chlorine " "		− 18,000	"
"	Peroxide of chlorine "	=	− 9,800	"
"	Chlorous acid "	=	− 21,000	"
"	Hypochlorous acid "	=	− 28,000	"

(Thomsen.)

These results show clearly that the stability of the series increases as we ascend, and not the reverse, as has been generally supposed, from the thermal values obtained by Favre. No known series of bodies, therefore, diminishes in stability, or has a regular increment of absorption.

3. On the Resemblances which Microscopic Objects in Dichroite and Amethyst have to some of the lower forms of Organic Life. By J. Scott, Tain. Communicated by Professor Kelland.

When examining with the one inch object-glass of a compound microscope some pieces of Strathpeffer Albert coal, I happened to place on the stage a crystal of dichroite, and was surprised to observe its surface covered with circular impressions. Their resemblance to some which I had previously noticed on iron pyrites associated with Albertite, led to a further inspection, which showed that they were due to globular bodies of various colours distributed throughout the crystal in layers parallel to the respective faces.

By means of sections cut parallel to these faces, I observed that the lower side of each layer, namely, that looking towards the interior of the crystal, differs essentially in its structural peculiarities from the upper. On that side each object has a conical form like a limpet shell, and usually consists of three or four easily

defined zones surrounding a well-marked central apex. With higher optical powers a greater number of segmentary zones are brought out, and radial and transverse striæ in exquisite detail.

Besides these individual characteristics, they exhibit composite relations of a peculiar kind, but in reality the development of a very simple principle. Whenever an increase of size has produced the contact of two or more individuals of a group, further enlargement has taken place by the formation of a common investing border.

From the deposition of the objects in successive layers, with their conical extremities resting on what had been at some stage of the crystal's formation one of its faces, they must have obtained their position whilst the crystal was in the act of formation.

Sections, either perpendicular or at an oblique angle to a face of the crystal, by presenting a side view of the objects, show that the hemispheroidal upper and the conical lower extremities were generally connected by a cylindrical body, whose comparative length varied considerably in the different individuals.

The same sections also exhibit a remarkable structural relation between the superimposed layers which occupy the successive laminæ of the crystal. When the objects can be traced from the interior towards the surface, they are found to have a linear arrangement symmetrically round axes perpendicular to the respective faces. These structural features can be well observed in sections through the terminal pyramid of some specimens of amethyst, in which they constitute a number of groups equal to the sides of the pyramid, each group consisting of a series of highly ornate beaded columns perpendicular to the same plane, and therefore parallel to one another.

The individual objects in the same axial line or column are often joined so closely that they may be considered as segments of a continuous whole, but in other instances the connection is a mere microscopic filament. In parts of the crystal a whole series of columns terminates on the same lamina, where the last segment of each has spread out to an extent which gives the structure the appearance of a disc with a long beaded handle attached to its centre. This last circumstance indicates one or both of two conditions—a period of retardation in the increase of the crystal, or of rapid acceleration in the growth of the objects. Irrespective of the

structural features which the individual objects possess, their mode of succession in linear directions perpendicular to the planes of the crystal displays a conformity to law, which could not have resulted from any chance deposition of coloured particles, whether solid or liquid, on the surface of the crystal during the process of formation. They must therefore belong to some peculiar crystalline form, or to some order in the organic world.

The mode of aggregation just described has obviously a close resemblance to some of those animal structures produced by continuous gemmation, as for instance some of the compound Foraminifera.

But the agreement between the objects and organic bodies is not confined to form and other structural resemblances—it extends to the changes through which they must have passed before they were enclosed in the substance of the crystal. Whatever their original nature, so completely have they become impregnated with the inorganic elements of the crystal, that the more opaque layers are often viewed through the silicified casts of their successors. Before, however, their condition became thus permanently fixed, there is evidence of continued and varied change. Specimens of amethyst contain whole layers from which the upper or globular end of each object has entirely disappeared, and their interior become occupied with silica so transparent that the delicate structural features of the conical extremity can be equally well seen when viewed on either side by transmitted light. In other instances nothing remains except portions of concentric annuli.

The objects that retain their structural features most entire are opaque, and as seen in a small section of dichroite appear of a brilliant white on the lower side, and of a somewhat silvery lustre on the upper. The same section also exhibits the changes on the external envelope, including its partial and complete removal. In one group it has disappeared from the one side, while it remains quite entire on the other, producing a well-marked boundary line, which passes over many of the individual objects, displaying in striking contrast the difference between the outside shell and the matter of the interior. Whenever divested of this covering, as is generally the case, the bodies are seen to be in groups of various colours, namely, red, orange, yellow, browns of various tints, and dark blue.

4. Note on the Zodiacal Light. By George Forbes, Esq.

A peculiarity was observed about the vernal equinox in 1871 in the shape of the zodiacal light, which deserves to be recorded. The appearance resembled a thin cone (such as is usually seen), extending to a great height, and rising out of a broad low cone situated at its base. This was not an effect of sunlight, for it was visible hours after sunset. It was not peculiar to any time or place, for it was seen constantly in all parts of the south of Europe, viz., in the Bay of Biscay, all along the Mediterranean, in Malta, and in Sicily. It seems not unlikely that there are periodic changes in the appearance of the zodiacal light. Hence it is well to mention any such peculiarity. I have also to confirm what has so often been stated by other observers, that the direction of the axis of the cone is not always in the direction of the ecliptic, but changes its direction from night to night.

Monday, 17th February 1873.

SIR ROBERT CHRISTISON, Bart., President, in the Chair.

The following Communications were read :—

1. Note on Ångström's Method for the Conductivity of Bars. By Professor Tait.

If we assume the excess of temperature above that of the air, v, to be the same throughout a transverse section of the bar, the equation for the flux of heat is—

$$\frac{dv}{dt} = \frac{1}{cp}\frac{d}{dx}\left(k\frac{dv}{dx}\right) - \frac{4h}{acp}v ,$$

where cp is the water equivalent of unit volume of the bar, k its thermal conductivity, a its side, and hv the quantity of heat lost by radiation and convection from unit surface of the bar per unit of time, when the excess of temperature is v.

Ångström writes this in the form—

$$\frac{dv}{dt} = K\frac{d^2v}{dx^2} - Hv,$$

assuming the conductivity to be unaffected by temperature, so that it is necessary that the range of temperature in his experiments be small. As the method consists essentially in so applying the heat as to bring the bar to a *periodic* state of temperature at each point, the solution must be of the form—

$$v = V + \Sigma_1^{\infty} A_n \epsilon^{-p_n x} \cos\left(n\frac{2\pi t}{T} - q_n x + \beta_n\right),$$

where T is the period, and V is the mean temperature or non-periodic part of the solution. Substituting in the equation, we have—

$$0 = K\frac{d^2V}{dx^2} - HV,$$

$$-\frac{2\pi n}{T} = -2Kp_nq_n,$$

$$0 = K(p_n^2 - q_n^2) - H.$$

The second of these is equivalent to Ångström's exceedingly simple expression for K in terms of the experimental data. Ångström, however, goes farther than this, for instead of the formula for v just given, he uses a more restricted one, which assigns very simple forms for the quantities p_n, q_n, viz.,—

$$p_n = g\sqrt{n}, \quad q_n = g'\sqrt{n},^*$$

where g and g' are absolute constants, depending on K, H, T alone. If this were admissible, it seems that we should have not only, with Ångström,

$$\frac{\pi}{T} = Kgg';$$

but also the impossible relation,

$$0 = Kn(g^2 - g'^2) - H,$$

* There are several serious misprints, both in the original (*Pogg.* 1862) and in the English translation (*Phil. Mag.* 1863, I.) In fact, in the expression for v, $\sqrt{x}$ appears instead of x, alike in the exponential and in the argument of the cosine.

where all the quantities are constants except n, which may be *any* positive integer.

It is obvious that, as the difference of the squares of p_n, q_n, is constant, while their product varies as n, their values will ultimately be *equal* for very great values of n. We may therefore assume, as an approximation, for large values of n—

$$p_n = \sqrt{\frac{\pi n}{\mathrm{KT}}}(1+e)\,,\ q_n = \sqrt{\frac{\pi n}{\mathrm{KT}}}(1-e)$$

where the square of e is negligible. This satisfies the first of our conditions, and the second gives

$$e = \frac{\mathrm{HT}}{4\pi n}\,.$$

This remark does not, of course, affect Ångström's deduction of the conductivity from the term of full period; but it must materially affect the others, and thus it is certainly remarkable how closely the results he has obtained from the second and third harmonic terms agree with these simple forms.

I have not as yet managed to control by this process my results deduced by Forbes's method (*Trans. R.S.E.*), though I hope soon to do so The difficulties are of two kinds: 1*st*, in the strictly periodic application of very hot sources, without a great range; 2*d*, in the procuring of thermometers which will give with great accuracy small differences at very high temperatures. In the meantime, with the assistance of several of my laboratory students, especially Messrs Greig and M'Leish, I have studied the periodic state of temperature in bars of copper (of two very different kinds), iron, and German silver, produced by applying for fifteen minutes at a time, and with fifteen minute intervals, a powerful Bunsen burner to one end. The ranges of temperature thus produced are so great that the differential equation assumed above can hardly be regarded as even a rough approximation to the law of the phenomenon. Still, the results possess considerable interest, especially in the contrast between the various substances; indicating the great differences not merely in conducting power, but also in its rate of change with temperature The bars employed were all 1¼-inch square, and the thermometers were inserted at 3-inch intervals.

Copper—C.

High Temperature.

I.	II.	III.	IV.
100 +	100 +	90 +	80 +
4·3	1·55	7·8	11·6
29·1	7·2	5·3	6·6
58·6	26·9	16·	10·7
78·7	43·	28·5	18·6
91·8	55·5	38·8	26·8
68·	51·4	41·6	32·1
41·	32·6	31·7	28·5
21·2	16·7	20·2	21·4

$A_0 =$ 149·087	$A'_0 =$ 129·33	$A''_0 =$ 113·737	$A'''_0 =$ 99·537
$A_1 = -$ 38·916	$A'_1 = -$ 25·95	$A''_1 = -$ 15·634	$A'''_1 = -$ 7·813
$B_1 =$ 7·688	$B'_1 = -$ 4·589	$B''_1 = -$ 8·874	$B'''_1 = -$ 9·453
$\alpha_1 =$ 39·668	$\alpha'_1 =$ 26·3524	$\alpha''_1 =$ 17·977	$\alpha'''_1 =$ 12·263
$\beta_1 =$ 168°·83	$\beta'_1 =$ 190°·02	$\beta''_1 =$ 209°·57	$\beta'''_1 =$ 230°·42

$A_2 = -$ 0·875	$A'_2 = -$ 0·612
$B_2 = -$ 0·7	$B'_2 = -$ 0·275
$\alpha_2 =$ 1·12	$\alpha'_2 =$ 0·68
$\beta_2 =$ 218°·65	$\beta'_2 =$ 240°·12
$A_3 = -$ 4·834	$A'_3 = -$ 1·025
$B_3 = -$ 1·112	$B'_3 = -$ 1·74
$\alpha_3 =$ 4·96	$\alpha'_3 =$ 2·02
$\beta_3 =$ 192°·95	$\beta'_3 =$ 239°·49

$\frac{\alpha_1}{\alpha'_1} =$ 1·5052	$\Delta\beta_1 =$ 21°·19
$\frac{\alpha'_1}{\alpha''_1} =$ 1·4658	$\Delta\beta'_1 =$ 19°·55
$\frac{\alpha''_1}{\alpha'''_1} =$ 1·4659	$\Delta\beta''_1 =$ 20°·85
$\frac{\alpha_3}{\alpha'_3} =$ 2·455	$\Delta\beta_3 =$ 46°·44

Copper—C.

Low Temperature.

I.	II.	III.	IV.
40 +	40 +	40 +	40 +
32·4	21·	12·15	5·
27·	20·6	13·9	7·1
18·7	15·7	11·45	6·65
12·5	10·8	8·	4·5
7·55	6·8	5·	2·4
13·55	7·3	3·65	·6
22·5	12·9	6·5	1·35
29·	18·1	10·05	3·7

$A_0 =$ 60·4	$A'_0 =$ 54·25	$A''_0 =$ 48·83	$A'''_0 =$ 43·91
$A_1 =$ 11·507	$A'_1 =$ 7·1915	$A''_1 =$ 3·9618	$A'''_1 =$ 1·6576
$B_1 = -$ 1·489	$B'_1 =$ 1·7606	$B''_1 =$ 2·687	$B'''_1 =$ 2·6154
$\alpha_1 =$ 11·603	$\alpha'_1 =$ 7·4038	$\alpha''_1 =$ 4·787	$\alpha'''_1 =$ 3·097
$\beta_1 =$ 352°·63	$\beta'_1 =$ 373°·75	$\beta''_1 =$ 394°·14	$\beta'''_1 =$ 417°·63

$A_2 = -$ ·3125	$A'_2 = -$ ·2
$B_2 = -$ ·2375	$B'_2 = -$ ·25
$\alpha_2 =$ ·3925	$\alpha'_2 =$ ·335
$\beta_2 =$ 217°·24	$\beta'_2 =$ 231°·34
$A_3 =$ ·918	$A'_3 = -$ ·09156
$B_3 =$ ·4108	$B'_3 =$ ·3606
$\alpha_3 =$ 1·	$\alpha'_3 =$ ·372
$\beta_3 =$ 384°·10	$\beta'_3 =$ 464°·25

$\frac{\alpha_1}{\alpha'_1} =$ 1·5671	$\Delta\beta_1 =$ 21°·12
$\frac{\alpha'_1}{\alpha''_1} =$ 1·5466	$\Delta\beta'_1 =$ 20°·39
$\frac{\alpha''_1}{\alpha'''_1} =$ 1·5459	$\Delta\beta''_1 =$ 23°·49
$\frac{\alpha_3}{\alpha'_3} =$ 2·688	$\Delta\beta_3 =$ 80°·15

Copper—Crown.

High Temperature.

I.	II.	III.	IV.
100 +	100 +	80 +	80 +
64·1	55·5	53·5	37·8
36·2	34·4	39·4	30·2
16·6	17·7	25·7	19·8
2·2	4·	14·2	10·2
32·	15·	15·3	6·6
60·	36·2	29·3	14·6
79·1	53·1	43·	24·6
92·6	65·1	54·	33·6

$A_0 = 147·85$	$A'_0 = 135·285$	$A''_0 = 114·3$	$A'''_0 = 102·15$
$A_1 = 19·798$	$A'_1 = 20·696$	$A''_1 = 18·371$	$A'''_1 = 14·694$
$B_1 = -35·8127$	$B'_1 = -20·05$	$B''_1 = -9·5398$	$B'''_1 = -2·58$
$\alpha_1 = 40·9152$	$\alpha'_1 = 28·8153$	$\alpha''_1 = 20·7006$	$\alpha'''_1 = 14·9187$
$\beta_1 = 298°·94$	$\beta'_1 = 315°·91$	$\beta''_1 = 332°·56$	$\beta'''_1 = 350°·05$

$A_2 = ·1$	$A'_2 = -·075$
$B_2 = ·35$	$B'_2 = ·375$
$\alpha_2 = ·47$	$\alpha'_2 = ·382$
$\beta_2 = 74°·05$	$\beta'_2 = 101°·57$
$A_3 = -3·748$	$A'_3 = -·358$
$B_3 = -4·563$	$B'_3 = -2·27$
$\alpha_3 = 5·905$	$\alpha'_3 = 2·298$
$\beta_3 = 230°·65$	$\beta'_3 = 261°·48$

$\frac{\alpha_1}{\alpha'_1} = 1·42$	$\Delta\beta_1 = 16°·97$
$\frac{\alpha'_1}{\alpha''_1} = 1·392$	$\Delta\beta'_1 = 16°·65$
$\frac{\alpha''_1}{\alpha'''_1} = 1·3875$	$\Delta\beta''_1 = 17°·49$
$\frac{\alpha_3}{\alpha'_3} = 2·455$	$\Delta\beta_3 = 30°·83$

Copper—Crown.

Low Temperature.

I.	II.	III.	IV.
40 +	30 +	30 +	30 +
·1	9·6	8·2	7·
7·2	12·	8·6	5·9
14·4	17·4	12·1	8·45
19·7	21·9	15·6	11·2
23·6	25·4	18·65	13·65
16·9	23·	18·7	13·7
9·65	17·7	15·	11·45
4·	13·	11·1	8·65

$A_0 = 51·94$	$A'_0 = 47·5$	$A''_0 = 43·49$	$A'''_0 = 40·$
$A_1 = -10·365$	$A'_1 = -7·4678$	$A''_1 = -5·1934$	$A'''_1 = -3·492$
$B_1 = 2·248$	$B'_1 = -·42123$	$B''_1 = -1·715$	$B'''_1 = -1·678$
$\alpha_1 = 10·606$	$\alpha'_1 = 7·5856$	$\alpha''_1 = 5·4692$	$\alpha'''_1 = 3·874$
$\beta_1 = 167°·76$	$\beta'_1 = 183°·22$	$\beta''_1 = 198°·27$	$\beta'''_1 = 205°·66$

$A_2 = -·0875$	$A'_2 = -·025$
$B_2 = ·1$	$B'_2 = -·025$
$\alpha_2 = ·132$	$\alpha'_2 = ·035$
$\beta_2 = 131°·19$	$\beta'_2 = 135°·0$
$A_3 = -1·38$	$A'_3 = -·432$
$B_3 = -·127$	$B'_3 = -·296$
$\alpha_3 = 1·39$	$\alpha'_3 = ·523$
$\beta_3 = 185°·25$	$\beta'_3 = 214°·42$

$\frac{\alpha_1}{\alpha'_1} = 1·398$	$\Delta\beta_1 = 15°·46$
$\frac{\alpha'_1}{\alpha''_1} = 1·387$	$\Delta\beta'_1 = 15°·05$
$\frac{\alpha''_1}{\alpha'''_1} = 1·412$	$\Delta\beta''_1 = 7°·39$
$\frac{\alpha_3}{\alpha'_3} = 2·663$	$\Delta\beta_3 = 29°·17$

Iron.

I.	II.	III.	IV.
130 +	110 +	80 +	70 +
16·2	6·6	10·	2·6
35·1	9·7	7·9	·9
52·3	17·	9·3	·1
65·65	24·5	12·3	·7
62·	29·9	15·85	2·1
41·5	28·1	17·7	3·6
25·	21·	16·9	4·1
8·2	12·5	13·85	3·7

I.	II.	III.	IV.
A_0 = 168·24	A_0 = 128·66	A_0 = 92·95	A_0 = 72·225
A_1 = − 22·737	A_1 = − 11·1989	A_1 = − 2·921	A_1 = ·178
B_1 = 15·85	B_1 = − 2·1312	B_1 = =3·906	B_1 = − 2·007
α_1 = 27·7161	α'_1 = 11·49	α''_1 = 4·8773	α'''_1 = 2·015
β_1 = 145°·12	β'_1 = 190°·77	β''_1 = 233°·20	β'''_1 = 275°·07
A_2 = ·225	A_2 = − ·375		
B_2 = ·68	B_2 = ·2		
α_2 = ·716	α'_2 = ·425		
β_2 = 71°·69	β'_2 = 151°·93		
A_3 = − ·163	A_3 = − ·451		
B_3 = 2·2	B_3 = − ·1314		
α_3 = 2·21	α'_3 = 1·47		
β_3 = 94°·24	β'_3 = 196°·24		

$\frac{\alpha_1}{\alpha'_1}$ = 2·413	$\Delta\beta_1$ = 45°·65
$\frac{\alpha'_1}{\alpha''_1}$ = 2·335	$\Delta\beta'_1$ = 42°·43
$\frac{\alpha''_1}{\alpha'''_1}$ = 2·421	$\Delta\beta''_1$ = 41°·87

German Silver.

I.	II.	III.	IV.
120 +	100 +	70 +	
79.4	39·1	13·8	60·
58·5	34·6	16·4	59·
29·6	25·5	15·45	57·3
10·2	14·5	11·9	55·7
20·7	6·5	7·1	55·15
48·5	9·5	4·1	56·05
73·1	20·	4·8	57·65
91·65	32·	8·4	59·3

I.	II.	III.	IV.
A_0 = 170·83	A_0 = 122·71	A_0 = 80·24	A_0 = 57·52
A_1 = 29·957	A_1 = 15·6806	A_1 = 3·2804	A_1 = 2·37
B_1 = − 24·39	B_1 = 2·7185	B_1 = 5·4555	B_1 = − ·2024
α_1 = 38·63	α'_1 = 15·915	α''_1 = 6·340	α'''_1 = 2·378
β_1 = 320°·85	β'_1 = 368°·84	β''_1 = 419°·37	β'''_1 = 475°·12
A_2 = − ·65	A_2 = ·025		
B_2 = ·0375	B_2 = − ·6		
α_2 = ·651	α'_2 = ·6		
β_2 = 176°·7	β'_2 = 358°·62		
A_3 = − ·607	A_3 = ·62		
B_3 = − 2·64	B_3 = − ·0315		
α_3 = 2·708	α'_3 = ·62		
β_3 = 256°·75	β'_3 = 357°·1		

$\frac{\alpha_1}{\alpha'_1}$ = 2·427	$\Delta\beta_1$ = 48°
$\frac{\alpha'_1}{\alpha''_1}$ = 2·510	$\Delta\beta'_1$ = 50°·53
$\frac{\alpha''_1}{\alpha'''_1}$ = 2·665	$\Delta\beta''_1$ = 55°·75

The thermometers were read once a minute when the periodic state was arrived at, the corresponding curves were traced; and from the curves so drawn, eight values of the temperature were deduced for successive intervals of three and three-quarter minutes. It was easy from these to calculate the coefficients of the harmonic terms up to the fourth inclusive, in the following expression—

$$v = A_0 + A_1 \cos\frac{2\pi}{T}t + A_2 \cos 2\frac{2\pi}{T}t + \dots$$

$$+ B_1 \sin\frac{2\pi}{T}t + B_2 \sin 2\frac{2\pi}{T}t + \dots .$$

From these again were calculated sets of values of α and β by the formulæ

$$\alpha = \sqrt{A^2 + B^2}, \quad \tan\beta = \frac{B}{A}.$$

In the preceding tables the dashes refer to the position on the bar, the suffixes to the order of the harmonic. It will be seen that the co-efficients of the even harmonics are too small to give any trustworthy results.

As the thermometers were read *successively* by one observer, the whole process occupying twenty seconds or $\frac{1}{90}$ of a period, the values of the phase must be diminished by 0°, 1°, 2°, 3°, respectively, *i.e.*, the differences of phase must each be diminished by 1°.

2. On the Thermal Conductivity of Ice, and a new Method of Determining the Conductivity of Different Substances. By Professor George Forbes.

The value of the coefficient of conductivity for ice is an important desideratum in several branches of physics; and it derives additional importance from an application, explained in the second part of this communication, to the determination of the thermal conductivity of different substances in absolute measure. The brilliant researches of Neumann,* and some ingenious experiments by M. Lucien De La Rive,† afford us at present the only two determinations that we possess of this important quantity. The value found by Neumann is 0·114, while De La Rive makes it 0·138. The discrepancy justifies the publication of experiments on a somewhat large scale, which gives us a close approximation to the truth.

Sir William Thomson suggested the method of imitating the freezing of a lake by means of a freezing mixture, and to deduce the conductivity from measures of the thickness of the ice formed in a definite time. In order to carry out this idea, I ordered an apparatus to be constructed by means of which a disc of ice could be formed twelve inches in diameter by a freezing mixture placed above a vessel of water kept constantly at 0° C. A means was devised for measuring the thickness of ice formed at successive intervals of time. The freezing mixture was drained constantly during the course of each experiment by means of a syphon. Temperature was read frequently at the base of the vessel in which it was contained. It was found possible to read the thickness of ice formed to within $\frac{1}{80}$th of an inch. The experiments lasted from four or five hours to twenty-one hours, a watch being kept continually on the drainage and temperature of the mixture for the first six or eight hours in experiments of long duration. The ice formed was quite uniform, very clear, and when cloven by planes perpendicular to the plane of freezing, split easily, showing the crystalline structure with great clearness.

Six whole days of frosty weather were employed in perfecting and completing the series of observations, during which time seventy-two readings were taken, capable of giving a value for the conductivity;

* Phil. Mag. 1863. † Soc. de Ph. d'Hist. Nat. de Genève, 1864.

but the early determinations were rejected for obvious reasons, and the ultimate determination was made from a mean of fourteen readings, the experiments having been performed in this case with extraordinary care, and with all the experience derived from previous trials.

Let the heat required to raise 1 gramme of water 1° C. be taken as our unit of heat. Assuming that in the formation of the ice a statical state of temperature has been reached, we have

$$F = k\frac{\delta\theta}{x}$$

when F = the flux of heat, k = the coefficient of conductivity, $\delta\theta = -$ (the temperature of the freezing mixture),

x = the thickness of ice.

k is assumed to be the quantity of heat which crosses an area of ice 1 square centimetre section in 1 minute, the thickness of ice being 1 centimetre, and the difference of temperature of the two sides of the ice being 1° C.

But F = the quantity of water raised 1° Cent., in 1 minute, over a surface of 1 sq. centimetre.

= (volume of ice formed in centimetres) × (latent heat of water) × (specific gravity of ice).

$$= \frac{dx}{dt} \cdot \text{S.L.}$$

$$\therefore\ k\frac{\delta\theta}{\cancel{x}} = \text{S.L.} \cdot \frac{dx}{dt}$$

$$\therefore\ k\,\delta\theta \,.\, t = \text{S.L.}\,\frac{x^2}{2}$$

or

$$k = \frac{\text{S.L.}\,x^2}{2\,\delta\theta \,.\, t}.$$

Employing this formula in the series of experiments alluded to above as being worthy of the greatest confidence, fourteen values of k were found corresponding to different values of x. Now, for small values of x an error in the value of x will introduce an error into the value of k greater than for large values of x. The value

of an observation is almost exactly proportional to x. After giving to each result a weight proportional to x, the mean thus obtained was

$$k = 0.134.$$

This is given in terms of the units mentioned above, to which also Neumann's and De La Rive's results have been reduced. Other experiments confirmed this result when reduced in the same manner.

In the course of these experiments some facts were noted, which, though not belonging exactly to the subject of this communication, are yet worthy of being recorded.

1*st*, A number of measurements were made in the temperature of salt and fresh snow, mixed in different proportions, with the following results:—

4	parts (by weight)	of	salt	+ 1	part	of snow	gives	− 20°·3 C.
2	„	„	„	+ 1	„	„	„	− 21°·1
1	„	„	„	+ 1	„	„	„	− 21°·4
1	„	„	„	+ 2	„	„	„	− 21°·66
1	„	„	„	+ 3	„	„	„	− 21°·72
1	„	„	„	+ 4	„	„	„	− 21°·4

2*d*, The blocks of ice formed were frozen in a cylinder, with air above and water below. These blocks were cut out by means of a chisel and hammer. In spite of the great force used, there was not the slightest tendency in the ice to split when thus compressed by the walls of the containing vessel, although the finest point split the ice with great ease when the block had been cut out.

Having completed the investigation with respect to ice, it occurred to me to extend the same process by means of a slight modification to the conductivity of other substances. The method employed was as follows:—A tin canister of about 3 inches diameter was filled with a freezing mixture whose temperature was frequently read. This was placed above the substance to be examined, which itself was laid in a flat tin dish resting on supports in water cooled to 0° cent. If the substance examined were a powder or soft material like cotton wool, it was made to rise to the level of two pieces of glass laid on the flat dish. So soon as ice began to be formed, it was considered that the statical state of temperature was

reached; accordingly the ice was then scraped off from the bottom of the flat dish, and the time noted. An experiment usually lasted about one hour. The thickness of the substance was measured and also that of the ice formed.

Let θ_1 be the temperature of the ice-cold water, θ_0 that of the freezing mixture, and θ that of the boundary between the substance examined and the ice formed.

Let x = the thickness of ice formed,
a = " substance examined,
k = conductivity of ice,
k_1 = " the other substance.

Then the flux of heat being F,

$$F = k\frac{\theta - \theta_0}{a} = k_1\frac{\theta_1 - \theta}{x}$$

$$= \frac{\theta_1 - \theta_0}{\frac{a}{k} + \frac{x}{k_1}} = \frac{\delta\theta}{\frac{a}{k} + \frac{x}{k_1}}$$

Also, adopting the same notation as before,

$$F = \frac{dx}{dt} \cdot \text{S.L.}$$

$$\therefore \frac{\delta\theta}{\frac{a}{k} + \frac{x}{k_1}} = \frac{dx}{dt} \cdot \text{S.L.}$$

$$\therefore \frac{t\delta\theta}{\text{S.L.}} = \frac{a}{k}x + \frac{x^2}{2k_1} \cdot$$

$$k = \frac{ax}{\frac{t\delta\theta}{\text{S.L.}} - \frac{x^2}{2.k_1}}$$

The last term in the denominator is always small for a non-conductor, and if ever it becomes large, we may be sure that this mode of experimenting is not available, since the temperature will not be in a permanent state.

In examining solid bodies, it is well to immerse the solid body itself in the water. Moreover, in this case I employed a convenient vessel to contain the freezing mixture, consisting of a funnel-shaped

glass vessel, 4 inches diameter at the top and 3 inches at the bottom, and being open at the two ends; its narrow end was closed by a tightly-stretched membrane, thus securing good contact.

In this way a large number of bodies was examined, with following results :—

Ice, along ax. . =	·134
Ice, perp. to ax. =	·128
Black marble . .	·106
White marble . .	·0691
Slate	·0486
Snow	·0432
Cork	·0430
Glass	·0300
Pasteboard . .	·0272
Carbon	·0243
Roofing felt . .	·0201
Firwood (parallel to fibre)	·0180
„ (across fibre and along the radius)	·00529
Boiler cement . .	·00975
Paraffin	·00843
Sand (very fine) .	·00788
Sawdust	·00736

Kamptulikon . .	·00660
Vulcanised India-rubber	·00534
Horn	·00522
Beeswax . . .	·00522
Felt	·00522
Vulcanite . .	·00500
Haircloth . .	·00241
Cotton wool (divided)	·00260
„ (pressed)	·00201
Flannel . . .	·00213
Coarse linen . .	·00179
Quartz, along axis .	·0553
„ „	·0745
„ „	·0340
„ „	·0498
„ Perpendicular	·240
„ „	·265

With regard to some of these substances, I may say that the white marble comes from Italy, though I know nothing more about it.

The slate is that commonly used for roofing.

The snow was frozen, and in consequence did not compress very evenly.

The cork was cut so that the conduction was along the fibre.

The pasteboard was the thick brown material often called mill-board.

The carbon was kindly lent me by Professor Tait.

The roofing felt was that commonly known as asphalte roofing felt.

The firwood was thoroughly seasoned.

The boiler cement is that supplied by Messrs Fleming, 23 St Vincent Place, Glasgow, and was kindly given to me along with several other materials.

The paraffin is that kind which has its boiling point at 45° C.

The sand was very fine, nearly pure silica, being that used for sand-baths.

The sawdust was that of common firwood, and was compressed.

The flannel was of the very coarse kind usually known as washing cloth.

The coarse linen was of the coarsest possible texture.

The quartz used for conduction along the axis was very thin. The piece used for conduction perpendicular to the axis was a large piece in the form of a hexagonal prism.

With regard to the numbers, I must say, in the first place, that they differ considerably from those of Peclet in nearly all the cases that admit of comparison. Reducing his numbers to the units employed above, we find—

Substance.	Forbes.	Peclet.
White marble	·0691	·463
Glass	·0300	·125
Carbon	·0243	·827
Caoutchouc	·00534	·028
Sawdust	·00735	·011
Cotton wool	·00530	·00666

It appears that there is a constant error due to the difference of methods. But it may be well to remark that Peclet has found very different results at different times, as may be seen at once by comparing the tables given at pages 355 and 481 of his "Traité de la Chaleur," 1843, vol. ii., and at page 406 of the second vol. of the same work as published in 1861. My experiments have at times varied, and I have given all the values I obtained for quartz, to show how injudicious it is to use a thin piece of a substance that is not a very bad conductor. The surface resistance is in that case too great to give good results. I intend to make further experiments on the conduction along the axis of quartz, which, along with a continuation of this investigation, I hope to have the honour of laying before the Society at a future time.

But these remarks do not apply to really bad conductors, and I have every reason to believe that the numbers given above do not differ widely from the truth.

The experiments on firwood confirm what we know about the difference in conduction along the different axes.

The experiments on cotton wool by no means refute what Peclet has found, viz., that the conduction is the same to whatever degree the wool is compressed, thus leading to the most interesting conclusion, that the conductivity of the fibre is the same as that of air, and that the conductivity of air is the number given above. The very low conductivity of many of these substances are proverbial; more especially flax, which we find at the bottom of the list. Horns and hoofs have also a bad name. The makers of boiler cement are well aware that they could have worse conductors, but they must consider the expense.

In all these experiments I was much assisted by Mr James Guthrie, one of my laboratory students.

3. On the Formation of Coal, and on the Changes produced in the Composition of the Strata by the Solvent Action of Water slowly percolating through the Earth's Crust during long periods of Geological Time. By R. W. Thomson, C.E., F.R.S.E.

(*Abstract.*)

The author commences by adverting to a very generally recognised geological difficulty—viz., that of accounting for the disappearance of the mineral from the carbonaceous matter in the processes which have resulted in the formation of coal-beds as we now find them. Coal-beds have undoubtedly their origin from decaying vegetable matter; and the deposition is unquestionably traceable to at least three different sources—viz., the carrying down by rivers of drift wood, and its deposition in deltas and estuaries at their mouths; the accumulations of dead forest trees, &c., falling for successive generations where they had grown; and the growth of peat. But in all of the three methods it is clear that the vegetable matter must have been mixed up to a very large extent with earthy matter, which earthy matter has since disappeared, so as to leave carbonaceous deposits in a comparative state of purity. The explanation of this disappearance has hitherto completely baffled geological ingenuity, and the

author, in offering the present solution of the enigma, ventures to hope that he has successfully grappled with the difficulty. Geologists appear hitherto to have strangely overlooked, or at all events very much underrated, the solvent powers of water in effecting changes on strata during inconceivably long periods of geological time, at great depths, and consequently under greater pressures and higher temperatures than are obtainable at the surface of the earth. The author contends that a just comprehension of the solvent action of water, slowly percolating through the strata during vast and nameless periods of time, and at pressures and temperatures of unknown intensity, will furnish the key to the elimination of the mineral from the carbonaceous matter which now constitutes our beds of coal. If carbon be not actually insoluble, it may be assumed that it is practically so in relation to any known chemical action and as compared with the proportionately easy solubility of the mineral ingredients, for the extension of which the present explanation is offered. Granting, therefore, the vast difference in degree of solubility, the immensity of the time since deposition, and the increased pressure and temperatures, with the incessant percolation of water through the strata, it is impossible to conceive any other result than the gradual washing out of the soluble from the insoluble constituents of any particular stratum. The different ingredients would disappear in the rotation of their degrees of solubility; and, in the case of coal-beds, the separation and carrying away of the mineral or soluble ingredients in solution and the leaving the carbonaceous or insoluble matter behind, would seem to be simply a question of time. The author touches upon several collateral features connected with the solvent action of water percolating through the strata, such as the deposition of chemically-dissolved matter in other strata through which the water has to pass at a lessened temperature, and consequently with a diminished power of solution, the vast supplies of shell-forming substances constantly being carried into the sea, and thus maintaining a supply sufficient for the formation of myriads of shells, and whole islands and almost continents of coral reef; and, in conclusion, he submits that the solution now offered, besides doing away with the principal difficulty, will contribute to the elucidation of many other obscure points in geology.

4. Note on Homocheiral and Heterocheiral Similarity. By Sir William Thomson.

Monday, 3d March 1873.

The Hon. Lord NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. On the Mud Banks of Narrakal and Allippey, two Natural Harbours of Refuge on the Malabar Coast. By George Robertson, Esq., C.E.

In the course of an examination of the harbours and river mouths of India, which I have recently been making for the Government of that country, instructions were sent me to examine the backwater communication of the Malabar coast, with special reference to the possibility of taking advantage of the anchorage at such localities as Narrakal and Allippey, and opening out communications between them and the backwaters. As these anchorages are so remarkable, and the phenomena connected with them are probably known to but few members of this Society, and the places themselves perhaps never visited by any member present, I have thought a short account of these mud harbours of refuge would be of some interest.

And, first, for a few words on the backwaters of Malabar. These consist of a network of lakes, river mouths, short rivers, and artificial cuts or canals, by which cargo boats can travel, with but one or two interruptions (now being overcome), from Buddagherry (to the north of Calicut), in lat. 11°·35, to Trevandrum, the capital of the state of Travancore, in lat. 8°·29. Eventually the system will be continued almost to Cape Comorin. The great value of this internal water communication is best shown in the south-west monsoon, when communication by sea is suspended for several months.

The south-west monsoon, which commences early in June, is a great bugbear to the commerce of the west coast of India, partly

because there are few harbours which can easily be entered in it, and ships lying out at sea could not communicate with the shore; and partly because the native craft are so ill found and such rattle-traps of vessels, that they would go to the bottom. But it is simply a long continuance of tolerably steady-blowing strong winds, with torrents of rain, such as we do not have in this country, and not the least formidable to properly-found vessels or to steamers. I have been at sea in the worst burst of the south-west monsoon, and never felt the slightest uneasiness, much less serious thought of danger.

The cyclones in the Bay of Bengal, on the east coast, are of course very different.

The rain, however, in the south-west monsoon is a serious draw-back to shipping cargoes.

I explored about 200 miles of the internal water-communication, reporting on Calicut, Beypore, and Cochin harbours for the Government of India, and also on Quilon, at the request of the Maharajah of Travancore, and the Dewan, Sir Madava Row. The canals in the northern position have only 2 feet 6 inches of water as a minimum, but in the Travancore state they are all intended to have 4 feet. I advised that the latter depth should be extended to the full distance, and the canals in various places straitened, so that the whole system might be opened up for small steamers, both to carry passengers and to tow cargo boats.

The greater part of the distance that Col. Farewell, superintending engineer, South Canara, and I travelled, was through cocoa-nut groves, and we had cabined boats, with from twelve to twenty rowers each, who sang fearful choruses almost the whole way.

The scenery was very beautiful, especially near Quilon, but the heat often very great, and always stifling. Of course, when we had to sleep in the boat, the mosquitoes were very troublesome.

The backwaters run parallel to the coast, and at a short distance from it, at times swelling out into lakes of varying sizes, the largest being at Cochin, where the backwater is almost the size of Loch Lomond. There are many mouths into the sea; some being shut up during the dry weather by the bars of sand which the surf is constantly throwing across any opening in the coast line; others, like

the entrance at Cochin, remaining open for navigation all the year round.

At Cochin there is always 11 feet of water at low water on the bar; but I hope that 20 feet will be attained by the works now projected, which will make Cochin by far the best harbour in the south of India.

The two mud banks I am going to describe are most valuable adjuncts to Cochin. Narrakal is about five miles to the north, and Allippey nearly forty miles to the south.

Allippey is the better known of the two, and I therefore take it first. In an old book (I believe on the voyages of Captain Cope) Allippey is mentioned in a way which proves that its peculiarities and advantages have been long known and appreciated. For this fact—and indeed for almost all I know of the mud bank—I am indebted to information obtained from Mr Crawford, the commercial agent for the state of Travancore, a shrewd Scotchman, who resides at the thriving and busy town of Allippey, and occupies a position of great responsibility. The bank is spoken of in that book as "Mud Bay," and described as one of the most extraordinary harbours in the world.

From Hamilton's account of the East Indies, in Pinkerton's collection of "Voyages and Travels" (1673 to 1723), it is thus spoken of, according to a report by Mr Maltby, the resident in 1860:—

"Mud Bay is a place that, I believe, few can parallel in the world. It lies on the shore of St Andrea, about half a league out in the sea, and is open to the wide ocean, and has neither island nor bank to break off the force of the billows, which come rolling with great violence on all other parts of the coast in the south-west monsoon, but on the bank of mud lose themselves in a moment; and ships lie on it as secure as in the best harbour, without motion or disturbance. It reaches about a mile along shore, and has shifted from the northward, in thirty years, about three miles."

There is some discrepancy in the statements of the movements of the bank in former times as to whether it moved from north to south or from south to north; but there can be no doubt about the last movement, since it has taken place during Mr Crawford's residence at Allippey.

In Lieutenant Taylor's chart of 1859, the bank is shown off the town of Allippey in lat. 9°·30. But, since then, it has moved four miles to the south, where it was when I examined the place, along with Mr Crawford, in March 1871. A letter from that gentleman in 1860 to the resident at Travancore gives the following account of this bank, with some of the theories which have been started in connection with its properties:—

"Lieutenant Taylor attributes the smoothness of the water to the soft mud at the bottom, by which, when 'stirred up by a heavy swell from seaward, the activity of the waves is so deadened as to render the shore-line free from surf.' I regret never having met Lieutenant Taylor.

"A number of years ago I brought to the notice of General Cullen, that the perfect smoothness of the water in the roads, and at the beach at Allippey, was attributable, not to the softness of the mud at the bottom so much as to the fact of the existence of a subterranean passage or stream, or a succession of them, which, communicating with some of the rivers inland and with the backwater, become more active after heavy rains, particularly at the commencement of the monsoon, than in the dry season, in carrying off the accumulating water, and with it vast quantities of soft mud. General Cullen, the resident, sent a quantity of piping and boring apparatus in order to test the existence, or otherwise, of what I had urged. Accordingly, I sunk pipes about 700 yards east from the beach, and at between 50 and 60 feet depth; and, after going through a crust of chocolate-coloured sandstone, or a conglomerate mixture of that and lignite, the shafting ran suddenly down to 80 feet; fortunately, it had been attached to a piece of chain, or it would have been lost altogether. Several buckets from this depth were brought up, which correspond in every respect with that thrown up by the bubbles as they burst at the beach, which I shall here attempt to describe as accurately as I can. Due west of the flagstaff, and for several miles south, but not north of that, the beach will, after or during these rains, suddenly subside, leaving a long tract of fissure, varying from 40 to 100 or 120 yards in length; the subsidence is not so quick at first; but, when the cone of mud once gets above the water, the fall is as much as 5 feet in some instances, when the cone bursts, throwing up immense quantities of soft soapy

mud, and blue mud of considerable consistence, in the form of boulders, with fresh water, debris of vegetable matter, decayed, and in some instances green and fresh. These bubbles are not confined to the seaboard, but are, I am inclined to think, both more active and numerous in the bed of the roads with the flagstaff bearing from E.N.E. to the south, until it bears N.E. by N., or even south of that. About five years ago, for about four miles down the coast, and from the beach out to sea for a mile and a half, the sea was nothing but *liquid* mud, the fish died, and as these cones reared their heads above the surrounding mud, they would occasionally turn over a dead porpoise and numerous fish; the boatmen had considerable difficulty in urging their canoes through this to get outside of it; the beach and roads presented then a singular appearance; nothing to be seen but those miniature volcanoes, some silent, others active; perfect stillness of all around the ships in the roads, as if in some dock, with a heavy sea breaking in seven fathoms outside.

"There are numerous deep holes, some of them I measured in 1852; one in particular, just at the end of this canal, had as much as 60 feet in depth; these holes may, or may not, communicate directly with the roads, but I think it will be found that the principal source of active communication is more inland, and the backwater perhaps only an auxiliary. About three miles above Chenganoor, in the river of that name, there are one or two deep "Linns" which I only had an opportunity of visiting twice; the first time I had not the means of ascertaining the depth, the next I lost both lead and line.

"The depth of this passage is not so great as you approach the beach, as noticed above; for, while extending the canal from the Timber Depôt in March last, about 200 yards from the beach, at 12 feet we suddenly and unexpectedly broke through the substratum, when a column, fresh water, mud, and vegetable debris, and about nine inches in diameter, spouted up, which, when left alone, gradually subsided as the upper stratum of sand filled in round the column of the spring.

"I submit the above information, as I feel that it will be interesting, both to yourself and Government, to pursue the investigation of this subject more efficiently. I have omitted to state one important particular,—that is, should *no* rain fall, as has been the

case this year, the sea in the roads and at the beach is not *nearly* so smooth; up to this time we have had none of the mud cones bursting at the beach, neither in the roads, as the waves tumble in perfectly clear; there was a heavy surf from the 26th ultimo to 9th instant, but never, in any instance, for these last eleven years, has the rain held off so long as in this, and the roads and beach have always, by the end of May, been perfectly smooth."

Since that letter was written, the bank has left Allippey, and has shifted some four miles down the coast, which considerably complicates the theories put forward. The oscillation of the bank —for it is said by some to have previously had a northward tendency—is very puzzling, viewed in connection with any supposed underground communication with the backwaters. The smoothness in rough weather is said to extend out to about the six fathom line. This is a point worth noticing, because, unless the mud suddenly stops there, it shows the extreme limit at which the waves on this coast have any effect on the bottom, even when that is composed of very fine slushy mud.

Mr Crawford states that the mud cones he describes take place only during the monsoon. At all events, I did not see any in action during my visit. He told me, likewise, that during the monsoon there is fresh water on the Allippey bank, but could not say whether it was succeeded by salt water to the north or south. This would require to be known, to make the existence of fresh water during the rains at all curious. It is possible that the fresh water may extend all the way to Cochin harbour entrance, and owe its existence either to that outlet on the north, or to some other outlet to the south.

Towards the formation of any theory about the Allippey bank, it will be useful to note that Mr Crawford has measured the level of the water in the backwater during the height of the monsoon, and found it to be 3 feet 2 inches above the sea-level. This gives a hydrostatic pressure from the backwater of less than a pound and a half on the square inch. The backwater is about two and a half miles from the sea at Allippey.

Mr Crawford, when I saw him, had an idea that volcanic action has something to do with the mud cones described by him, but was unable to overcome the difficulty I suggested, that it was only

regularly at one time of the year, and that during the rainy and windy season, that the mud cones appeared.

There is a curious circumstance about the Allippey bank, which I give also on Mr Crawford's authority, viz., that, in gales of wind, vessels may be seen lying at anchor on the bank with their bows pointed in various directions, as if influenced by eddying currents in the sea of mud.

The weather being too calm, and the time of year being too early, to exhibit the full virtues of the bank, I noticed none of these wonders, but I saw enough to show that there was some extraordinary virtue in the place. We got into the boat with some difficulty at Allippey, on account of the surf; but, at the place where the bank now is, for several miles there was not only no surf, but not a ripple at the water's edge, and we stepped on to the shore from the boat with the greatest ease. Looking from shore towards the sea horizon, one saw a crest of surf, or, more properly speaking, swell, all round in a horse-shoe form, and reaching out to about three and a half miles from land, enclosing this smooth pond,—the swell being gradually deadened as it neared shore, till it died off into absolute quiescence.

I passed Allippey in a P. & O. steamer last autumn during the height of the S.W. monsoon; but, although within sight of land, we were too far off to notice the peculiarities of the mud bank.

The *Narrakal* bank, at present five miles to the north of Cochin, has been known for long, but was almost forgotten till it was rediscovered (I may say) by Captain Castor, the master-attendant at Cochin in 1861, and surveyed by him in 1865. Captain Castor (who is a native and a very intelligent man) is now master-attendant at Coconada, but was ordered to meet me at Cochin, so that I had the advantage of his presence in visiting Narrakal. Curiously enough, he is now stationed at the only place I visited in India, which approached in character to the peculiarities of these mud banks; for, at Coconada, there is a quantity of mud in the bay, which to a considerable extent reduces the surf. But Coconada is a regular bay, into which the Godavery river discharges its mud; whilst the banks now in question are detached spots of a peculiarly

greasy mud, moving about on a straight coast, and away from any river mouth.

In some of the records of the late Captain Biden, the then master-attendant of Madras, is to be found the following remark about Narrakal (date 1841):—" This bank is situated at Pooryapooly about nine miles to the north of Cochin within the Cochin Circar's territories; the extent of it is about six miles, and the soundings from one to seven fathoms."

But there is a much earlier reference to Narrakal, in the translation of an old work by a Dutch navigator, called " Voyage to the Cape of Good Hope, Batavia, Samarang, Surat, East Indies, &c., in the year 1774 to 1778," book iii., cap. 12.

In describing Cochin, he says—" The coast is safe and clear everywhere along the Company's Establishment, except at the mouth of the river of Cranganore [about twelve miles to the north of Cochin]. South of the above-mentioned mouth of the river of Cranganore there is a bay formed of mud banks, the banks forming which extend to fully a league out to sea, and into which vessels may run with safety during the bad monsoon, and may lie in twenty and less feet of water, almost without anchors or cables, in perfect security against the heavy seas which then roll in upon this lee shore, as they break their force upon the soft mud banks, and within them nothing but a slight motion is perceived."

A better description of Narrakal could not be given than is given by this writer of a century ago, except that the action of the mud extends out to the six fathom line, and that the bank has shifted south to within five miles of Cochin.

I heard nothing about mud cones in connection with Narrakal;—either there were none, or there was no one to observe them.

I visited Narrakal in the pearl fishery steamer, the " Margaret Northcote," which had been lent to me for the cruise round Cape Comorin, and which drew only five feet of water, so that we were able to go through the Paumben Passage, and thus save the voyage round Ceylon. I may mention that this last autumn I visited Paumben again, to report on the proposed ship canal which will shorten the voyage from Europe and Bombay to the Bay of Bengal, by three and a half days on the double voyage. The day we visited Narrakal there was a considerable swell on, and its effects were

very marked on the countenance of the Prime Minister of Cochin, who had ventured on the sea, probably for the first time. In return for the compliment, he turned out the state snake-boats of the Rajah to attend us next day. They are very long narrow-framed canoes, each with some fifty rowers, who sit with their legs dangling over each side of the boat alternately; and they go at a great pace. The "bucksheesh" for so many men came to be heavy!

I sent a diver down for a specimen of the mud (which I regret to have lost). It is of a peculiarly greasy nature, dark green in colour, and sticky; a specimen was analysed at Madras in 1861, and was found to contain—

1. Very minute angular fragments of quartz, the largest hardly visible without a lens; this is the sand

2. Foraminiferous shells of the genus Rotalia, and a few fragments of larger shells.

3. Upwards of twenty genera of Diatomaceæ.

4. A few specules of sponges and corals, very minute.

5. Some amorphous matter, which was not destroyed after long boiling in strong acids.

I reported to the Government against attempting to open out communication between either of the mud banks and the adjoining backwater. A cut through the neck of land between would throw an outwards scour during ebb tides, and during the rains into the centre of the mud banks, and might do a great deal of harm, and indeed destroy them, or shift their position. During the dry season, at flood tide the mud would be drawn into the backwaters, and choke them up.

If a lock were to be put on the canal, the channel leading to it could not be kept clean without a scour.

At Narrakal there is already a canal from the backwater to within a few yards of the shore; and at Allippey there is a similar one, only the mud bank has left it and travelled south.

These natural harbours of refuge are too valuable to try experiments on; and I think the whole phenomena connected with them are *well worthy of careful scientific investigation.*

2. The Meteorology of the Month of May. By Alexander Buchan, M.A.

Excepting the months of January and July—the months of extreme temperatures for the larger portions of the globe—there is no month the meteorology of which is so peculiar, and a careful investigation of which is so likely to lead to striking and important results, as the month of May. The peculiarity of the meteorology of the month of May is, that it is the month of the year during which the most rapid rise of temperature takes place over the greater parter part of the northern hemisphere, and the most rapid fall over the greater part of the southern hemisphere; and since that rapid rise and equally rapid fall takes place at very different rates, according to the peculiar distribution of land and water in each region, the inquiry is calculated to bring out in strong relief some of the more prominent causes which influence climate, and some of the more striking results of those causes. The method of inquiry which has been adopted was to compare the average atmospheric pressure of May with that for the year, setting the difference of excess or deficiency in their proper places on maps, and drawing therefrom lines of equal deviation from the annual mean, for every 0·100 inch, and in some cases for 0·050 inch. The winds had been dealt with in a similar way—viz., by finding the difference between the average of May and the general monthly averages of the year. From these two elements—distribution of pressure and winds—the rainfall and other elements of climate necessarily follow. The results of the inquiry which has been made, and which was based upon observations at upwards of 600 places, show a diminution of pressure in May over tropical and sub-tropical regions, and also over the north of Asia and to the south of South America and Tasmania. The excess of pressure in the northern hemisphere prevails over North America (to the north of the Lakes), over Arctic America, over Greenland, over the British Isles, and to the north of a line passing through the English Channel, in a north-easterly direction, to the Arctic Sea. Excess in the southern hemisphere includes the southern half of South America and of Africa, the whole of Australia, and the adjacent parts of the ocean. The influence of land in the southern hemi-

sphere, where the land at this season is colder than the surrounding sea, brings about a higher pressure for May; but the influence of land over regions heated more immediately by the sun brings about a lower pressure—interesting examples of which are seen in the distribution of the differences of pressure over India, the Malayan Archipelago, the Mediterranean, Black, and Caspian Seas. In many such cases the lines follow more or less closely the contour of the coasts, or, more strictly speaking, the lines resembling the contours lay some distance to eastwards, so that there is a less diminution over those seas than over the land surrounding them. Nearly the whole of Asia shows a very large deficiency of pressure—the Arctic regions to the north of Europe and North America the maximum excess of pressure. It is to the position of Great Britain, with reference to the deficiency of pressure on the one hand, and the excess on the other, that the east winds at this time of the year are due. Those easterly winds prevail over the whole of northern Europe, as far south as a line drawn from Madrid in a north-easterly direction, and passing through Geneva, Munich, &c. To the south of that line the diminution of pressure is less, and over that region the excess of wind is, not easterly, but southerly. Crossing the Mediterranean, and advancing on Africa, we approach another region of lower pressure; and towards that region north-easterly winds again prevail, as at Malta, Algeria, &c. The effect of these different winds upon the rainfall is very decided. Southerly winds from the Mediterranean result in heavy rainfall over France and Central Europe in the month of May.

The effect of those east winds upon diseases is very great. They derange our nervous system, and bring about a series of complaints, physical and mental, an inquiry into which would form an interesting and important subject of investigation.

3. On Vortex Motion. By Sir William Thomson.

The following Gentlemen were elected Fellows of the Society:—

Andrew Pritchard, M.R.I., Author of a work on Infusoria, Highbury, London.

Walter Stewart, F.C.S., Haymarket Terrace.
Robert Tennent, Esq., 21 Lynedoch Place.
Robert Walker, M.A., Edinburgh Academy, Fellow of Clare College, Cambridge.
William Boyd, M.A., Peterhead.
Morrison Watson, M.D., Demonstrator of Anatomy in the University, Edinburgh.
J. Bell Pettigrew, M.D., F.R.S., Conservator of Museum, Royal College of Surgeons.

Monday, 17*th March* 1873.

Sir ALEXANDER GRANT, Bart., Vice-President, in the Chair.

The following Communications were read:—

1. A Contribution to the Visceral Anatomy of the Greenland Shark (*Læmargus borealis*). By Professor Turner.

Naturalists have recorded a few instances of the capture of the Greenland shark in the British seas. Dr Fleming states that one was caught in 1803 in the Pentland Firth, and that one was found dead at Burra Firth, Unst, in 1824. Mr Yarrell refers to a specimen caught on the coast of Durham in 1840, which has been preserved in the Durham University Museum. In May 1859, a specimen about ten feet long was caught in the Firth of Forth, near Inchkeith, the stuffed skin of which is preserved in the Edinburgh Museum of Science and Art. In 1862 a specimen was caught on the Dogger Bank, and brought into Leith. A brief description of its external character was read by Mr W. S. Young to the Royal Physical Society of Edinburgh. On April 27, 1870, Dr John Alexander Smith read before the same Society a notice of a female specimen caught about thirty miles east of the Bell Rock. It had become entangled in one of the deep-sea fishing-lines, many of the hooks attached to which had stuck into its body. It measured about 15 feet in length, and 3 feet 1 inch between the tips of the tail-lobes. The stuffed skin of this fish is also preserved in the Edinburgh Museum of Science and Art. In the month of February of the present year, three specimens were caught by fishermen at sea, some miles east of the Bell Rock, and brought into Broughty

Ferry. One was taken to Dundee for exhibition; the others were brought to Edinburgh for the same purpose. By permission of the proprietors, the author was enabled to examine the latter specimens, and to acquire for the Anatomical Museum the viscera and other parts. One was a large female, 11 feet 8 inches in length; the other, a smaller female, 8½ feet long. Colour, bluish-grey; sides of body marked with a number of transverse stripes; lateral line distinct. The author then recorded several measurements of the larger specimen, of which the more important were as follows:—

	Ft.	in.
From tip of snout to end of tail, . .	11	8
„ to back of 1st dorsal fin,.	6	0
„ to back of 2d dorsal fin, .	9	0
„ to antr. edge of ventral fin,	7	9
„ to antr. edge of pectoral fin,	3	5
Height of 1st dorsal fin, ...	0	7
„ 2d dorsal fin, . . .	0	5¼
Between tips of tail-lobes, . . .	2	9
Length of pectoral fin, ...	1	8
„ ventral fin, ...	1	3

A specimen of the parasitic crustacean, the *Lerneopoda elongata*, was attached to one of the eyes of the smaller specimen.

The author then gave an account of the visceral anatomy of this shark; all the measurements given being from parts of the larger specimen. The stomach possessed, in addition to the large sac, a posterior pyloric compartment from which the pyloric tube arose, which curving for 6 inches forwards, terminated by a very constricted orifice in the duodenum.

The duodenum was a cylindrical tube, 3 feet 2 inches in length. It ran at first forwards and then passed backwards to end in a dilated part of the intestine 13 inches long, which contained the transversely arranged spiral valve. A short rectum, 7 inches long, passed from the spiral valve back to the anus, and into this part of the gut the duct of an ovoid glandular body, attached to the outer coat of the rectum, opened. The biliary and pancreatic ducts pierced independently the wall of the duodenum, where it bent on itself, and between their opening and the pyloric orifice two large

cæca, one 6 inches, the other 18½ inches long, opened by wide mouths into the duodenum.

The pancreas was a well-developed organ, from which two long processes passed backward parallel to the duodenum. The bile-duct, for some inches before it joined the duodenum, was a single, well-marked tube, and had connected with it a small bilobed body, from which a minute duct, parallel to the bile duct, ran towards the liver. The spleen was 17 inches long and 6 wide at its broadest part. The kidneys lay parallel to the spine; their ureters, about the size of crow quills, opened into the cloaca behind the anus.

The ovaries were two in number, and each was 23 inches long in the larger shark. They consisted of parallel club-shaped laminæ, and contained multitudes of ova, varying in size from minute specks to small bullets. No oviducts were seen in the abdominal cavity, and no oviducal openings in the region of the cloaca; but immediately posterior to the mouth of the cloaca, the two rounded openings of the abdominal pores, which communicated with funnel-shaped prolongations of the peritoneal cavity were found.

The heart, with its subdivisions into auricle, ventricle, and conus arteriosus, was then described, and the structural differences between the conus and the bulbus aortæ of the osseous fish were pointed out.

The conus arteriosus of the heart, in addition to the large three-segmented, semi-lunar valve at its anterior end, contained four tiers of valves, consisting of nineteen cuspidate segments, to and from which, and from the inner wall of the conus, chordæ tendineæ proceeded.

It was then pointed out that the presence of a pyloric compartment, and of a cylindrical tubular duodenum, the co-existence of a pancreas and pyloric cæca, and the absence of oviducts, constituted most important features of difference between the Greenland shark and the other Plagiostomata.

Attention was then drawn to the differences in the form of the teeth, which had led Müller and Henle to separate the Greenland shark from the old Cuvierian genus *Scymnus*, and to make for it a new genus *Læmargus*.

The author then stated that the anatomical differences between

Scymnus and *Læmargus* were very much greater than those referable to the form of the teeth, on which systematic zoologists had hitherto relied in their classification. These differences, indeed, affected not only the relations of *Læmargus* to *Scymnus*, but to the sharks generally, and called for a reconsideration on the part of the zoologist of the place which the Greenland shark ought to occupy amongst the Plagiostomata, and required the establishment of a separate family for the reception of the genus *Læmargus*, which family would possess the following characters:—

LÆMARGIDÆ.

No nictitating membrane; two dorsal fins; no anal fin; duodenum cylindrical; both a pancreas and duodenal cæca; in the female no oviducts.

Læmargus.—Dorsal fins short, the second not quite so high as the first; lower teeth oblique, truncate.

2. Additional Note on the Strain-Function, &c. By Professor Tait.

The author gave an account of the mode in which he had treated the Strain-Function in an elementary Treatise on Quaternions, soon to be published, mainly from the pen of Professor Kelland.

The coefficients of the cubic in ϕ are determined easily from the condition that homogeneous strain alters the volume of every part of a body in the same ratio.

A careful examination is bestowed upon the case of three real roots of the cubic; especially with regard to the distinction between the results of a self-conjugate strain and a rotational one.

The separation of the pure and rotational parts of a strain is very fully treated; and as special examples, the strain of a rigid body and a simple shear are analysed.

Finally, the following problems are solved:—

Find the conditions which must be satisfied by the simple shear, which is capable of reducing a given strain to a pure strain.

Find the relation between two linear and vector functions whose successive application produces rotation merely.

All this is independent of the differential calculus, but as the following results regarding the stress-function require its aid, the

cannot be introduced into the work referred to. They will appear, with extensions, in the second edition (now printing) of the author's Treatise on *Quaternions*.

At any point of a strained body, let λ be the vector stress per unit of area perpendicular to i, μ, and ν, the same for planes perpendicular to j and k respectively.

Then, by considering an indefinitely small tetrahedron, we have for the stress per unit of area perpendicular to a unit vector ω, the expression

$$\lambda Si\omega + \mu Sj\omega + \nu Sk\omega = - \phi\omega ,$$

so that the stress across any plane is represented by *a linear and vector function* of the unit normal to the plane.

But if we consider the equilibrium, as regards rotation, of an infinitely small rectangular parallelepiped whose edges are parallel to i, j, k, respectively, we have

$$V(i\lambda + j\mu + k\nu) = 0 ,$$

or

$$\Sigma Vi\phi i = 0 ,$$

or

$$V . \nabla\phi\rho = 0 .$$

This shows that ϕ is *self-conjugate*, or, in other words, involves not nine distinct constants but only six.

Consider next the equilibrium, as regards translation, of any portion of the solid filling a simply-connected closed space. Let u be the potential of the external forces. Then the condition is obviously

$$\iint \phi(U\nu)ds + \iiint d\varsigma\nabla u = 0 ,$$

where ν is the normal vector of the element of surface ds.

Here the double integral extends over the whole boundary of the closed space, and the triple integral throughout the whole interior.

To reduce this to a form to which the method of my paper on *Green's and other Allied Theorems* (*Trans.* R.S.E., 1869-70) is directly applicable, operate by $S.\alpha$ where α is any constant vector whatever, and we have

$$\iint S.\phi\alpha U\nu ds + \iiint d\varsigma S\alpha\nabla u = 0,$$

by taking advantage of the self-conjugateness of ϕ. This may be written

$$\iiint d\varsigma(S.\nabla\phi\alpha + S.\alpha\nabla u) = 0\,,$$

and, as the limits of integration may be any whatever,

$$S.\nabla\phi\alpha + S.\alpha\nabla u = 0 \quad . \quad . \quad . \quad . \quad (1).$$

This is the required equation, the indeterminateness of α rendering it equivalent to *three* scalar conditions

As a verification, it may be well to show that from this equation we can get the condition of equilibrium, as regards *rotation*, of a simply connected portion of the body, which can be written by inspection, as

$$\iint V.\rho\phi(U\nu)ds + \iiint V.\rho\nabla u d\varsigma = 0\,.$$

This is easily done as follows:—(1.) Gives

$$S.\nabla\phi\sigma + S.\sigma\nabla u = 0,$$

if, and only if, σ satisfy the condition,

$$S.\phi(\nabla)\sigma = 0\,.$$

Now this condition is satisfied if

$$\sigma = V\alpha\rho\,,$$

where α is any constant vector. For

$$\begin{aligned} S.\phi(\nabla)V\alpha\rho &= -\,S.\alpha V\phi(\nabla)\rho \\ &= \quad S.\alpha V\nabla\phi\rho = 0\,. \end{aligned}$$

Hence

$$\iiint d\varsigma(S.\nabla\phi V\alpha\rho + S.\alpha\rho\nabla u) = 0\,,$$

or

$$\iint dsS.\alpha\rho\phi U\nu + \iiint d\varsigma S.\alpha\rho\nabla u = 0\,.$$

Multiplying by α, and adding the results obtained by making α in succession each of three rectangular vectors, we obtain the required equation.

Suppose σ to be the displacement of a point originally at ρ, then the work done by the stress on any simply connected portion of the solid is obviously

$$W = \iint S.\phi(U\nu)\sigma ds\,,$$

because $\phi(U\nu)$ is the vector force overcome on the element ds.

This is easily transformed to

$$W = \iiint S.\nabla\phi\sigma d\varsigma\,.$$

Monday, 7th April 1873.

PROFESSOR SIR WILLIAM THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. Notice of a Singular Property exhibited by the Fluid enclosed in Crystal Cavities. By Edward Sang, Esq.

The subject of the following communication is a phenomenon unexpected and peculiar; it presents analogies to the phenomena of magnetism and electricity, in so much as it is an exhibition of repulsion; but it is distinguished from these by the absence of attraction, or what is called polarity. So far as I am aware, it is the only known example of repulsion exhibited independently of magnetic or electric excitement, and seems to open up an entirely new field for physical research. On these accounts I was exceedingly desirous to have it brought without delay to the notice of scientific men, and I have to thank our Secretary for giving me the present opportunity, although at the inconvenience to him of it having to accompany a long and interesting paper on another subject.

I shall confine myself this evening to a simple statement of the nature of the phenomenon, to its exhibition, and to an account of the circumstances that led to its discovery, reserving for another opportunity a more detailed notice of those observations and experiments that have already been made in regard to it.

While discussing, along with Dr James Hunter, the occurrence of polished cylindric striæ in calcareous spar, and while examining those striæ under the microscope, I happened to notice an air-bubble in a minute cavity, having a regularly crystallised form, which air-bubble was found to move when the position of the spar was changed.

Next forenoon, while showing this, in itself very interesting, matter to my pupil, Mr David A. Davidson, I desired to mark the position of the speck, and applied the point of my penknife to scratch the spar. Immediately the air-bubble was seen to move rapidly. My first thought was to attribute this motion to the pressure on the thin lamina of spar immediately above the cavity, but

on exerting a much greater pressure by help of a small pencil of wood, no motion was perceptible, yet on bringing the knife-point within the field of view, but without pressure, the motion was renewed, and the bubble was seen to approach the steel on whichever side it might be placed. The possible slight magnetism of the steel suggested itself as an explanation, accompanied, however, by the unheard-of occurrence of a magnetised fluid; and the blade was magnetised first in the one and then in the opposite way without any perceptible change of effect. Meanwhile Mr Davidson had found that a piece of soft steel occasioned the same motions. Pieces of brass and copper wire, printing type, silver and copper coin, all acted in the same way; but pieces of wood, glass, ivory, showed no effect.

Afterwards, trials made with compact oxide of iron, and with sulphuret of lead, gave no perceptible result; yet, until the trials shall have been conducted with scrupulous care as to the horizontality of the upper surface of the cavity, we cannot hold the absence of action to be proved; but, as present appearances go, it seems that the metallic state is essential to this repulsion.

By inclining the instrument, we may bring this repulsion to oppose gravity, and the degree of inclination affords a test of the amount, so much so that means for determining the law of its variation by distance, and the specific influences of different metals, are brought within reach.

Modifications in the arrangement of the microscope, so as to allow of the convenient exposition of specific masses, as well as to secure the measurements of the inclination and distance, are needed before we can obtain results reliable as to quantity; when these modifications are completed I shall place the details before the Society.

Roughly made, as at present, the experiments point to a specific intensity for each metal, and to a diminution in a ratio higher than that of the inverse squares of the distances.

I have not been so fortunate as to find among the cavities in rock crystal, topaz, and amethyst, within my reach, any containing movable fluid: it is desirable that physicists, who may be in possession of such specimens, should examine whether this repulsion occur there also.

2. On the Germ Theory of Putrefaction and other Fermentative Changes. By Professor Lister.

The following Gentlemen were elected Fellows of the Society:—

JOHN G. M'KENDRICK, M.D., Assistant to the Professor of Physiology in the University of Edinburgh.
ROBERT WILSON, Esq., Engineer, Patricroft, Manchester.

Monday, 21st April 1873.

PROFESSOR KELLAND in the Chair.

The following Communications were read:—

1. Notice of New Fishes from West Africa:—
(I.) *Ophiocephalus obscurus*, Günther.
(II.) *Synodontis Robbianus*, nov. spec. mihi.
By John Alexander Smith, M.D.

The fishes now exhibited were brought by the Rev. Alexander Robb, D.D., from Old Calabar, West Africa. They were taken in the fresh water of the great Old Calabar River, near Ikorofiong, about a hundred miles or so, by the windings of the river, from the bar near its mouth. The Rev. Dr Robb resides at Ikorofiong, which is one of the stations of the Calabar mission of the United Presbyterian Church.

The fishes belong to the great SUB-CLASS of the TELEOSTEI.

I. *Ophiocephalus obscurus*, Günther.

The first to which I would call attention is a small dark-coloured fish; it belongs to the Order of the ACANTHOPTERYGII, Family OPHIOCEPHALIDÆ, and to the Genus *Ophiocephalus.*

Dr Günther, in his "Catalogue of Acanthopterygian Fishes," vol. iii. p. 468, states that the fishes of this family have the *body elongate, anteriorly sub-cylindrical,* and covered with scales of moderate size; the head depressed and snake-like, covered with shield-like scales superiorly. *A cavity accessory to the gill cavity, for the purpose of retaining water in it,* a superbranchial organ, not

being developed. *One long dorsal and anal fin without spines.* "They are fresh-water fishes of the East Indies, and are able to live and move without the water for a short time, feeding on small animals." "It appears, from recent observations, that the amount of air which is in solution in water is not sufficient for the respiration of these fishes, so that they are obliged to come to the surface at certain intervals, to receive an additional quantity of atmospheric air."

The genus *Ophiocephalus* is distinguished by the presence of ventral fins. The species of this genus are common in India and the East; some of them, as the "*Cora mota*" or "*Gachua*" (the *O. gachua*) of Bengal, have excited considerable interest from making their appearance during the rains in unexpected places, and giving rise to the popular belief that they must have fallen with the rain from the clouds; the fish having left, for the time, the muddy waters where it resides, for the fresh wet grass, and the abundance of animal food it gets there.

This genus was believed to be entirely confined to India and the East until Dr Günther, in the year 1869, detected in the collection of fish made by Consul Petherick on the Nile one species which he has described as the *O. obscurus*. It was captured at Gondokoro on the Upper Nile, and forms the only exception yet known to the Indian habitat of the genus.

The interesting fact of the great apparent correspondence of the fish fauna of the Nile with the distant rivers of West Africa was pointed out many years ago; the fauna of the East African rivers being apparently somewhat different in character. Dr Günther, from a careful examination of a number of species from the Nile and West African rivers, comes to the conclusion that—"the Faunæ of the Nile and the West African rivers belong to the same zoological district; that there is an uninterrupted continuity of the fish fauna from west to east; and that the species known to be common to both extremities inhabit also the great reservoirs of water in the centre of the African continent."*

It is, therefore, with some little interest that I am able to add this single species of *Ophiocephalus* found in the Upper Nile, to the

* See Petherick's "Travels in Central Africa," vol. ii. London, 1869. Appendix, "Fishes of the Nile," by Dr A. Günther.

list of corresponding species found in the great Calabar river of Western Africa. The specimen seems to correspond very closely with Dr Günther's typical description of the *O. obscurus*, with the exception of some slight proportional details of measurements and the presence of one or two more rays in some of the fins. I forwarded the fish to Dr Günther for his examination, and he writes me that "the *Ophiocephalus* is closely allied to, if not identical with, the *obscurus*, but it has five or six more dorsal rays than the type." We must, therefore, perhaps, wait for the examination of additional specimens, to see whether some of the characters will require to be expanded a little, in Dr Günther's description of the fish.

(Since this paper was read to the Society, Dr Günther informs me that the British Museum has recently received a specimen from the river Congo, with thirty anal rays.)

I subjoin Dr Günther's description of the Nile fish, taken from the appendix to Petherick's "Travels in Central Africa," vol. ii. London, 1869, p. 215. Dr Günther had, however, previously described and named this fish in his general "Catalogue of Acanthopterygian Fishes," vol. iii., London, 1861, p. 478, from a specimen in the collection of the British Museum, the locality of which was not known:—

"*Ophiocephalus obscurus*, A. Günther.

D. 42. A. 26–29. (L. lat. 70. L. trans. 7/14.)

"The height of the body is nearly one-eighth of the total length, the length of the head nearly one-fourth; the width of the inter-orbital space is more than the extent of the snout, and one-fourth of the length of the head. The cleft of the mouth is wide, the maxillary extending behind the orbit. The scales on the upper surface of the head are of moderate size, those on the neck small; there are thirteen series of scales between the orbit and the angle of the preoperulum. The pectoral does not extend on to the origin of the anal, and its length is one-half that of the head; the length of the ventral is three quarters of that of the pectoral. Caudal rounded, its length being six times and one-third in the total. Blackish, lighter below, with dark stripes along the series of scales; a series of black blotches along the side; head with two indistinct oblique black spots along its base. Pectoral and ventral variegated with blackish. Chin black, with white spots. Length seventy-seven lines. Collected at Gondokoro."

The following are some of the slight differences in the specimen got in the Old Calabar River:—

Ophiocephalus obscurus, A. Günther.

D. 45. A. 32. P. 16. V. 6. (L. lat. about 70. L. trans. 7/14.)

Height of body, $7\frac{1}{2}$ times in total length of fish. Length of head, $4\frac{1}{2}$ times in total. The length of the pectoral fin is a little more ($\frac{1}{8}$ of an inch), than half the length of head. Length of ventrals rather more than half that of pectorals. Caudal fin is $5\frac{1}{4}$ times in the total length. Head and body above are black, or a very dark brown (in spirits), the sides show numerous black blotches; fins black, tail slightly mottled with lighter. Below, head black, blotched with lighter, rest of body dirty white. Total length, 78 lines ($6\frac{1}{2}$ inches.)

Collected at Ikorofiong, Old Calabar River, West Africa.

II. *Synodontis Robbianus*, nov. spec. mihi.

The other fish belongs to the Order of the Physostomi, Family Siluridæ, and to the Genus *Synodontis*. All the species of this genus belong to tropical Africa, and at least one species has been discovered common to the Upper Nile and the West African rivers. They are scaleless fish, with an adipose fin, and the dorsal and pectoral fins have strong bony spines. Mouth small, Teeth in the lower jaw movable, very thin at the base, and with slightly dilated brown pointed apices. They have six barbels, and broad dermal bones on the head and neck. I have taken these details of the characters of the genus from Dr Günther's important work, the "Catalogue of Fishes," vol. v., to give a general idea of the fish, and the following are the character of this new species:—

Synodontis Robbianus.

Body.—Height (behind dorsal fin), about one-fourth of length without caudal rays. Greatest height; one-third of distance between posterior border of orbit, and caudal extremity without fin rays. *Head* about three and a half times in length of body, without caudal fin; tapers quickly forwards; short in front of eyes; distance from point of snout to front of orbit, about one-third of length from snout, to posterior extremity of nuchal plate. Snout short, rounded in front; distance between middle of orbits rather less than to front of snout. The gill openings extend downwards to before the root of humeral process of pectoral fin.

Teeth:—Mandibular, rather numerous, much shorter than the eye (about half), varying in length, the longest towards the middle; in a cluster on

1. *Synodontis Robbianus* (natural size).
2. Mouth, showing teeth and roots of barbels.

J. M. Corner, Del.

the middle of front of lower jaw. *Maxillary* teeth in front of upper jaw, small, short, thickly set in a broad band.

Barbels:—Maxillary barbels, dark-coloured, much longer than head, reaching more than half way down pectoral spines; edged with a broadish membrane interiorly. *Mandibular barbels.—Outer*, dark-coloured, slightly fimbriated or fringed, reaching to base of pectoral fin. *Inner*, light-coloured, about half the length of outer, and more distinctly fringed.

Dermal bones of head and neck.—Broad, rough or granular; terminate in front of eyes in forked processes; broad behind, and extend in a pointed process a little beyond each side of base of dorsal spine. *Humeral process.*—Much longer than high, pointed behind, runs nearly as far back as nuchal plate, granular surface, a slight projecting ridge along anterior margin, and a thick, somewhat smooth, and tapering ridge projects along its inferior border.

Fins rather small:—D. 1/8. A. 12. P. 1/7. (V. 7.)

Dorsal fin.—Spine shorter than the head (fixed upright), almost smooth in front, showing only some very obscure indications of a few short processes or teeth at upper part; toothed at upper part behind, teeth directed somewhat towards base of spine; (a small soft ray or filament inserted a little below the point.) First five rays (the third the longest) as long as spine and filament together. *Adipose fin.*—Elevated; longer than head; space between it and dorsal, about equal to length of base of dorsal fin without spine. *Pectoral fin.*—Spine larger and longer than dorsal, toothed on both sides; teeth small and thickly set together on outside, directed towards extrémity of spine; teeth larger and more apart on inner side, and point towards base of spine; fin reaches a little beyond base of dorsal fin, but not to base of ventral fins. *Ventral fins.*—Small; in length pass anal opening, but do not reach to base of anal fin. *Anal fin.*—Larger than ventral.

Tail.—Forked nearly half the depth of rays; two uppermost rays produced about a third beyond others.

Colour (in spirits).—Pale brown, slightly blotched or mottled with darker, especially on head, at insertions of fins and tail, and on rays of fins and tail. Ventrals and anal fin nearly black. Spines light coloured.

Total length of fish without caudal rays, $4\frac{3}{8}$ inches; to extremity of elongated caudal rays, $5\frac{5}{8}$ inches. From point of snout to posterior extremity of nuchal plate ($1\frac{7}{8}$ inches), fully a third of total length to extremity of elongated caudal rays. Total length of specimen, $5\frac{5}{8}$ inches.

Captured at Ikorofiong, Old Calabar River, West Coast of Africa.

I have named the fish after the Rev. Alexander Robb, D.D., to whom I am indebted for these specimens, as well as various others, from the Old Calabar district of tropical Africa.

Dr Robb tells me there are great difficulties in the way of getting

specimens of natural history of almost any kind in Old Calabar; and one in particular depends on the fact, that the natives eat at once all they can capture, and are most unwilling to give them up for any other than their own gastronomic purposes.

The fishes of this genus *Synodontis*, and the allied genera, are interesting to the geologist from their possessing dermal bony plates, and also these strong bony fin-spines, which are analogous in character to some of those in the fossil fishes, and to the *ichthyodorulites*, or fin spines, which are found fossil in many of our older rocks.

These bony spines are useful to the fish as weapons both of offence and defence, and require a very careful handling of some of the species, which grow to a considerable size, as they sometimes inflict serious wounds, which are said to be poisonous, even in some cases causing death. Dr Robb says, the dangerous character of the fish of this genus is well known to the Old Calabar natives, as well as, doubtless, to some of the animals which prey upon fish. Crocodiles are abundant in the river, and in some instances make a seizure of one of these fishes with the large bony spines, and cases have occurred of a crocodile being found dead with the spiny fish sticking in its mouth or throat. This circumstance has probably given rise to an Efik proverb well known among the people, to this effect,—"When the Crocodile is lucky, he catches *Inanga*" (the spineless cat fish); "when unlucky, he catches *Mkpi-kuk-i-kuk*" (the native name for this spiny fish or *synodontis*), the etymology of which, Dr Robb tells me, is not very obvious. The proverb, indeed, wonderfully resembles our own common saying about "catching a Tartar," and is frequently used by them in its more general application, as among ourselves.

2. The Electrical Conductivity of certain Saline Solutions, with a note on their Density. By J. A. Ewing and J. G. MacGregor, B.A. Communicated by Professor Tait.

(*Abstract*).

In the note on the density of the solutions prepared for the purpose of determining their electrical conductivity, it is shown that the ratio of the weight of salt dissolved in unit weight of water to the

excess of the density of the solution thus formed over that of water (unity), is not constant, but increases, with greater or less rapidity, from the more dilute to the more dense. The work of previous experimenters on the electrical resistance of liquids is then reviewed at some length. Their chief difficulty has always been the electrolytic polarisation of electrodes.

The solutions under investigation were put in a glass tube, which was narrow along the central part, but widened at the ends for the reception of platinum electrodes; and by means of connecting wires it was made to form one of the arms of a Wheatstone's Bridge. High resistances were introduced into the other arms. The bridge was so arranged that the effect on the galvanometer could be observed the instant the battery circuit was closed, when for an indefinitely short period there is no polarisation. By successive passages of electricity, which were alternately in opposite directions, and between which the tube was short-circuited, opportunity was obtained of adjusting the resistances in the arms of the bridge, so that at last there was no deflection of the galvanometer needle due to the passage of the current. All measurements and observations were made at a temperature of 10° C.

Nineteen solutions of zinc sulphate were examined. The resistances of very dilute ones are very great, but fall off rapidly as the density increases, until it reaches about 1·08, after which they decrease much more slowly. At the density 1·2891, the specific resistance (*i.e.*, the resistance between opposite faces of a cube, whose side is 1 cm.) is 28·3 B.A. units. The resistances of solutions from this density to that of saturation increase, that of the saturated solution being 33·7 B.A.U. That solution, therefore, whose density is 1·2891, is the solution of maximum conductivity. By taking as ordinates the excess of the density of the various solutions over unity, and as abscissæ their specific resistances, the relation between density and resistance is shown graphically. The curve thus obtained is symmetrical, about an axis passing through the point of maximum conductivity, and the part of it which lies between the origin and that point is an hyperbola whose asymptotes are inclined at an angle less than a right angle.

Eleven solutions of copper sulphate were prepared and their

resistances found. The spec. res. of the saturated solution is 29·3 B.A.U. The solution of maximum density is that also of maximum conductivity. The curve (described as above) is an hyperbola whose asymptotes are inclined at an angle less than a right angle.

Nine solutions of potassium bichromate, and nine also of potassium sulphate, were investigated. The resistances in both decrease up to the point of saturation, the least being, in the former, 29·6 B.A.U., and in the latter, 16·6 B.A.U. The curves are both hyperbolas; that of the bichromate has its asymptotes approximately perpendicular to one another, and that of the sulphate has them inclined at an angle greater than a right angle.*

Fifteen mixtures of equal volumes of solutions of copper and zinc sulphates were also examined. In all cases, the resistance of the mixture is less than the mean of the resistances of its components. The lowest spec. res. of any of tho mixtures prepared, is 27·3 B.A.U., and it consisted of equal volumes of the two saturated solutions. The zinc sulphate appears to exercise the greater influence in the determination of the spec. res. of the mixtures.

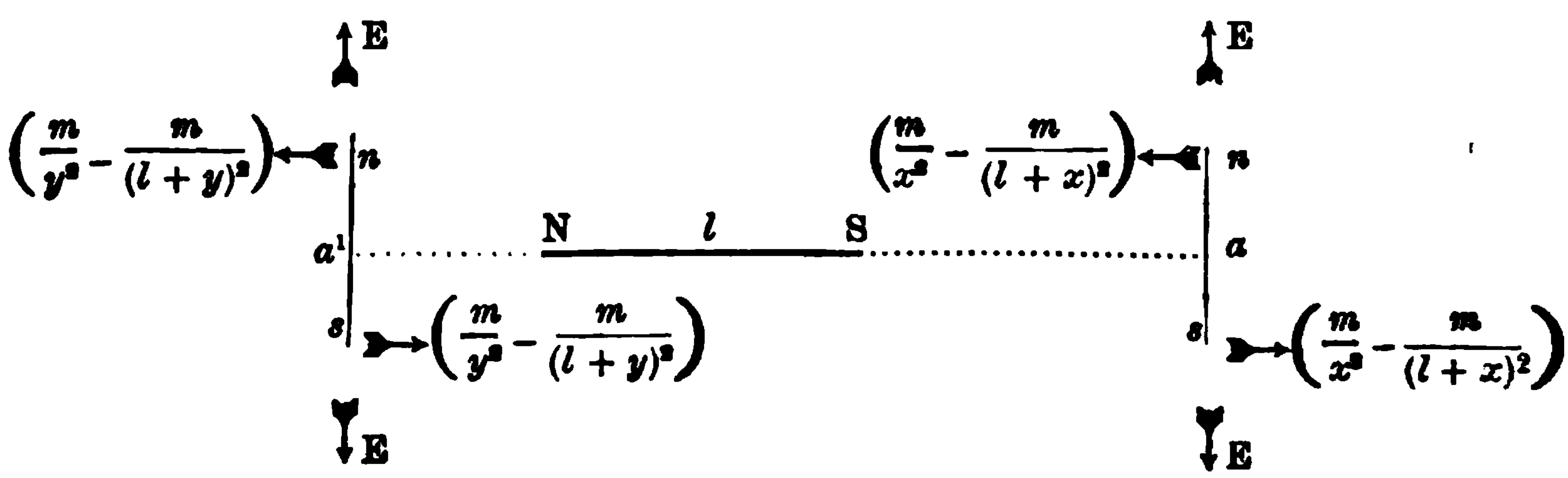
3. On the Effect of Heating one Pole of a Magnet, the other being kept at a Constant Temperature. By D. H. Marshall, Esq., M.A., and C. G. Knott, Esq. Communicated by Professor Tait.

The following are a modification of some experiments conducted in the summer of 1871, and communicated to the Society on the 15th of January 1872. These consisted in heating a magnet uniformly throughout, and then noting the change in magnetic strength. Those conducted this winter consisted in heating one pole of a magnet, while the other was kept at a temperature as nearly constant as possible, and then noting the change of magnetic strength in both poles. The arrangement adopted was the same in both series of experiments, only being double in the latter. It consisted in having a magnet set magnetically east and west, each end of which passed through a cork fitted into a hole made

* For all four salts formulæ are given, by means of which the conductivity of any solution may be calculated if its density is given.

in the side of a copper pot, one of which was filled with oil and heated by means of a brass Bunsen, while the other was filled with water at the temperature of the air of the room. The temperatures of both ends of the magnet could thus be ascertained by means of mercurial thermometers.

In the same line as the magnet and on both sides of it were two small magnets. These were cemented to the backs of small concave mirrors, suspended by single silk fibres, and placed in glass cases to guard them against currents of air. The deflections of the small magnets were measured exactly as in the reflecting galvanometer; and from the nature of the arrangement, and the important fact made out from these experiments, viz., that even when the poles of a magnet are at different temperatures the magnetic strength is the same in each, it follows that the absolute magnetic strength in either pole of the large magnet is directly as the tangent of the angle of deflection of the contiguous small one, and, therefore, will be measured by the reading on the corresponding scale.

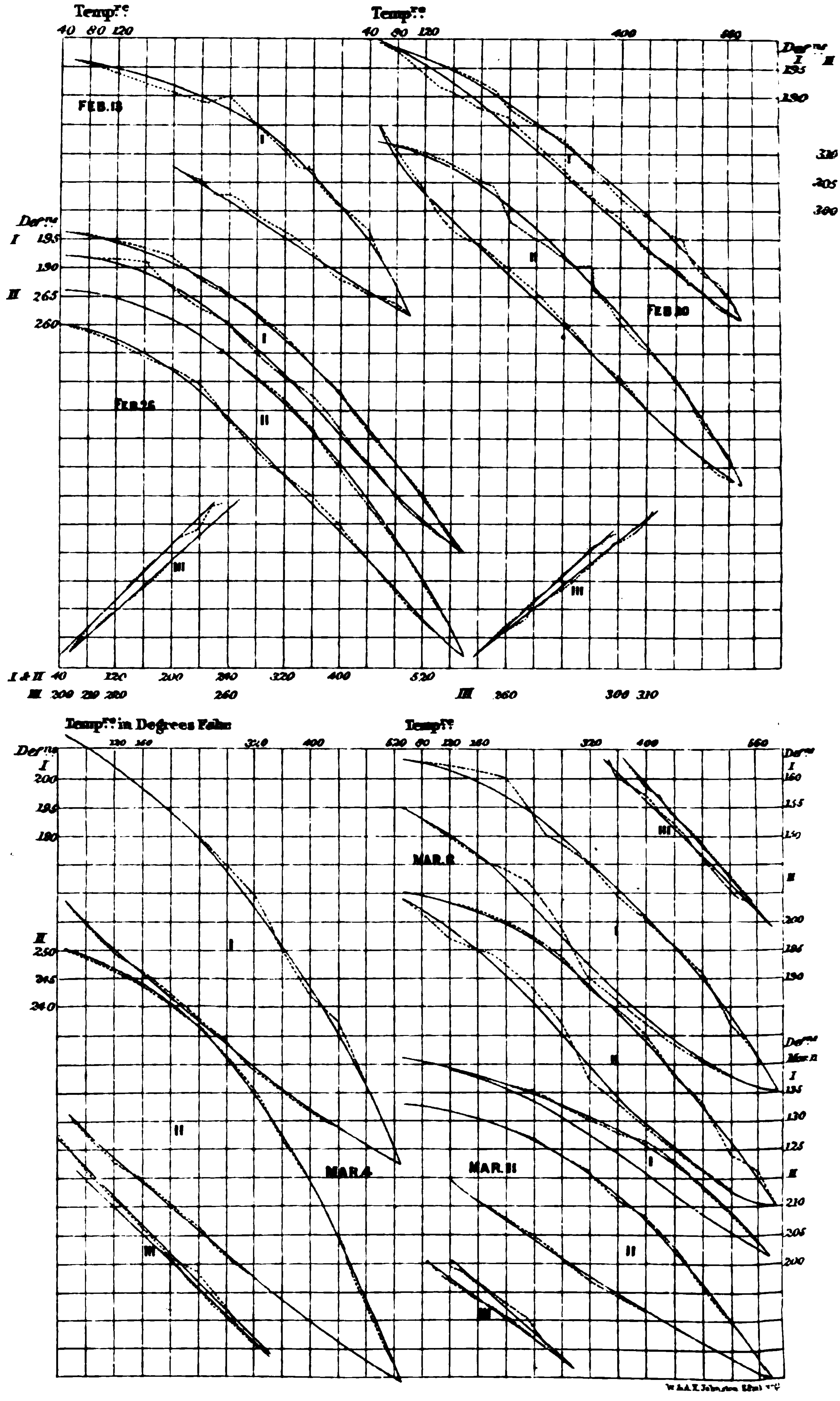


NS are the poles of the fixed magnet, m its absolute magnetism. N $a = x$, N $a^1 = y$, $\mathrm{NS} = l$. The couples indicated are those produced by the large magnet and the earth's magnetism, E, on the small magnets.

For any deflection θ, if the lengths of the small magnets be negligible compared with x and y, we have

$$\mathrm{E} \sin\theta = m\left(\frac{1}{x^2} - \frac{1}{(l+x)^2}\right)\cos\theta \;\therefore\; m \;\alpha\; \tan\theta.$$

$$\mathrm{E} \sin\theta = m\left(\frac{1}{y^2} - \frac{1}{(l+y)^2}\right)\cos\theta \;\therefore\; m \;\alpha\; \tan\theta.$$



Temp.re
Temp.re
Deg.res
FEB.13
FEB.20
FEB.26
I
II
III
I & II 40 120 200 240 320 400 520
III 200 210 220 260
III 260 300 310
Temp.re in Degrees Fahr.
Temp.re
MAR.4
MAR.8
MAR.11

Curve I. of each day's experiment represents the rate of diminution of the hot pole in terms of its temperature; Curve II. that of the cold pole in terms of the temperature of the hot pole; and Curve III. shows how the magnetic strength of the one pole changes relatively to that of the other.

Perhaps the most important fact made out from these experiments was that already mentioned, viz., that whatever be the temperatures of the ends of the magnet, the magnetic strength is approximately the same in both poles. This was shown by plotting the deflections of the little magnets in terms of one another; for it was then found that the result was a straight line, which proves that the rate of diminution or increase of magnetic strength is the same in both poles; and, therefore, if they be of the same strength at the commencement of each experiment, when the magnet is of the same temperature throughout, they will of course remain the same during the whole experiment, when the poles are of different temperatures. (See figure III. of each day's experiment.) When the pole which had been heated was allowed to cool again, the line obtained by plotting the deflections of the two poles was still approximately a straight one, though not exactly coinciding with that obtained for it when being heated. This, however, is quite satisfactorily accounted for by the alteration of the zeros, which was never the same in both, and therefore the measured strength differed from the true strength more in one pole than in the other. In the first communication it was pointed out how the mere heating of the pot produced in some unaccountable way an alteration of zero. The fact that the poles are the same strength throughout also accounts for the general similarity of Curves I. and II. for each day.

It is to be observed that Curves I. and II. become smoother day after day, as if the boiling so altered the molecular constitution of the steel, as to enable it to conduct more easily a state of magnetic distribution.

It is interesting also to notice that the rate of diminution of magnetic strength decreases on each successive experiment, and that the two branches of each curve approach nearer and nearer to each other, thus showing a greater unwillingness on the part of the magnet to lose any of its magnetism permanently with heating.

The curves of March 4 represent an experiment in which the poles of the magnet were reversed, *i.e.*, what was before heated is now kept at the temperature of the room; and these curves show what a very great permanent loss was produced by the reversion, and also how much greater the rate of diminution of the magnetism was than had been in previous experiments. In after experiments, such as that of March 6, the permanent loss is not nearly so great, and there is also a remarkable decrease in the rate of diminution.

The curves of March 11 represent an experiment in which the poles were again reversed; and, from the approach of the branches, we see that the magnet, as it were, becomes accustomed also to this treatment, and evinces great unwillingness in losing its magnetism permanently. In this day's experiment there is a great dissimilarity between Curves I. and II. We may look for the explanation of this in the alteration of zeros, which possibly may be much greater in the one pole than in the other. This supposition is strengthened on looking at Curve III., where it will be seen the two branches are very widely separated.

Throughout these experiments we have to acknowledge the assistance which W. Campbell, Esq., has rendered us.

4. On the Physiological Action of Light. No. I. By James Dewar, Esq., and John G. M'Kendrick, M.D., of the University of Edinburgh.

The authors of this communication have more especially directed their attention to the problem of the specific effect produced on the retina and optic nerve by the action of light. Numerous hypotheses have been made from time to time by physicists and physiologists; but up to the present date, our knowledge of the subject is without any experimental foundation. For example, Newton, Melloni, and Seebeck, stated that the action of light on the retina consisted of a communication of mere vibrations; Young conjectured that it was a minute intermittent motion of some portion of the optic nerve; Du Bois-Reymond attributed it to an electrical effect; Draper supposed that it depended on a heating effect of the

choroid; and Mosier compared it to the action of light on a sensitive photographic plate.

It is evident that, in accordance with the principle of the transference of energy now universally accepted, the action of light on the retina must produce an equivalent result, which may be expressed, for example, as heat, chemical action, or electro-motive power. It is well known that the electro-motive force of a piece of muscle is diminished when it is caused to contract by its normal stimulus, the nervous energy conveyed along the nerve supplying it; and similarly a nerve suffers a diminution of its normal electromotive force during action. In the same manner, the amount and variations of the electro-motive power of the optic nerve, affected secondarily by the action of light on the retina, are physical expressions of certain changes produced in the latter; or, in other words, are functions of the external exciting energy, which in this case is light. Considerations such as these led us to form the opinion that the problem of what effect, if any, the action of light has on the electro-motive force of the retina and optic nerve, would require for its investigation very careful and refined experiment.

The inquiry divided itself into two parts,—first, to ascertain the electro-motive force of the retina and nerve; and, second, to observe whether this was altered in amount by the action of light. The electro-motive force of any living tissue can be readily determined by the method of Du Bois-Reymond. This great physiologist found that every point of the external surface of the eyeball of a large tench was positive to the artificial transverse section of the optic nerve, but negative to the longitudinal section. This he accomplished by the use of his well-known non-polarisable electrodes, formed of troughs of zinc carefully amalgamated, containing a solution of neutral sulphate of zinc, and having cushions of Swedish filter paper on which to rest the preparation. To protect the preparation from the irritant action of the sulphate of zinc, a thin film or guard of sculptor's clay, moistened with a ·75 per cent. solution of common salt, and worked out to a point, is placed on each cushion. These electrodes were connected with a galvanometer, and the preparation was placed so that the eyeball, carefully freed from muscle, rested on the one clay-guard, while the transverse

section of the optic nerve was in contact with the other. By following Du Bois-Reymond's method, we have had no difficulty in obtaining a strong deflection from the eyes of various rabbits, a cat, a dog, a pigeon, a tortoise, numerous frogs, and a gold-fish. The deflection was frequently so much as to drive the spot of light off the galvanometer scale.

With regard to the second question, namely, whether, and to what extent, the electro-motive force would be affected by light, we found more difficulty. The method followed was to place the eyeball on the cushions in the manner above described, to note the deflection of the galvanometer needle, and then to observe whether or not any effect was produced on the impact of a beam of light, during its continuance, and on its removal. In a few of our earlier experiments, we used Du Bois-Reymond's multiplying galvanometer; but finding the amount of deflection obtained was so small that the effect of light could not be readily observed, we have latterly used Sir W. Thomson's exceedingly sensitive reflecting galvanometer, kindly lent us by Professor Tait. We met also with secondary difficulties, such as the dying of the nerve, the impossibility of maintaining an absolutely constant zero and an absolutely constant amount of polarity, the effects of heat, &c.; but these difficulties we have overcome as far as possible by the most approved methods. The changes in polarity of the apparatus occurred slowly, and could not be mistaken for the changes produced by the action of light, which we found occurred suddenly, and lasted a short period of time. It is also important to state, that the deflections we observed do not at present profess to be absolute, but only relative values.

About 500 observations were made previous to the date of this first communication, and we took every precaution to obtain accurate results. The effects of heat were carefully avoided by covering over the troughs on which the eye under examination rested, with a spherical double shell of glass, having at least an inch of water between the walls.

The results we have arrived at are as follow:—

1. The action of light on the retina is to alter the amount of the electro-motive force to the extent of from three to seven per cent. of the total amount of the natural current.

2. A flash of light, lasting the fraction of a second, produces a marked effect.

3. A lighted match, held at a distance of 4 or 5 feet, is sufficient to produce an effect.

4. The light of a small gas-flame, enclosed in a lantern, and caused to pass through a globular glass jar (12 inches in diameter), filled with a solution of ammoniacal sulphate of copper or bichromate of potash, has also produced a change in the amount of the electro-motive power.

5. The action of light on the eye of the frog is as follows:—When a diffuse light is allowed to impinge on the eye of the frog, after it has arrived at a tolerably stable condition, the natural electro-motive power is in the first place increased, then diminished; during the continuance of light it is still slowly diminished to a point where it remains constant; and on the removal of light, there is a sudden increase of the electro-motive power nearly up to its original position. The alterations above referred to are variables, depending on the quality and intensity of the light employed, the position of the eyeball on the cushions, and modifications in the vitality of the tissues.

6. Similar experiments made with the eye of warm-blooded animals, placed on the cushions as rapidly as possible after the death of the animal, and under the same conditions, have never given us an initial positive variation, as we have above detailed in the case of the frog, but always a negative variation. The after inductive effect on the withdrawal of light occurs in the same way.

7. Many experiments have been made as to effect of light from different portions of the spectrum. This was accomplished by causing different portions of the spectrum of the oxy-hydrogen lime-light to impinge on the eye. All these observations tend to show that the greatest effect is produced by those parts of the spectrum that appear to consciousness to be the most luminous, namely, the yellow and the green.

8. Similarly, experiments made with light of varying intensity show that the physical effects we have observed vary in such a manner as to correspond closely with the values that would result if the well-known law of Fechner was approximately true.

9. The method followed in these inquiries is a new method in physiological research, and by the employment of proper appliances, it may be greatly extended, not only with regard to vision, but also to the other senses.

Monday, 5th May 1873.

Professor KELLAND, V.P., in the Chair.

The following Communications were read:—

1. Notice of two Fossil Trees lately uncovered in Craigleith Quarry, near Edinburgh. By Sir R. Christison, Bart., President, R.S.E.

The late Mr H. T. M. Witham read in 1830 to this Society, and published three years afterwards in greater extension, an inquiry of much interest respecting two fossil trees found in Craigleith Quarry, a mile and a half from the north-west outskirts of Edinburgh. The general points of this inquiry are, that trees of very great size lie, completely fossilised, in the very compact sandstone of the quarry, at a great depth below the rock surface, slightly inclined to the dip of the strata, with their structure so finely preserved in the fossilising material as to be beautifully shown before the microscope, and recognised as that of the Pinaceous Family, and of the section to which belongs the existing Araucaria. These trees have been generally known to fossile botanists by the name of *Araucarioxylon Withami*. An opportunity having occurred this year of confirming and extending the inquiries of Witham, it has been thought right to take advantage of it, again through the medium of the Royal Society.

One of Witham's fossils (No. 1) was found in 1826, the other (No. 2) in 1830; but his researches regarded principally the second. Since the latter year four similar fossils have been uncovered by the operations of the quarrymen. One of these (No. 3) was exhibited for some time to the curious in a hut constructed over it for concealment. Another (No. 4) was removed by the late Mr Ramsay of Barnton, behind whose mansion several large

fragments may still be seen. The fifth is said to have been first brought to light in 1858, and to have been subsequently covered with the detritus of the quarry, till it was lately uncovered again by the operations of the quarry having returned to its neighbourhood; and it now lies in its place half displayed to the extent of 22 feet. The sixth has been found only a few months ago in the very bottom of the quarry, where, for the present, little more is seen of it than a cross section, level with the containing rock. There was also lately found, not far from the second last fossil, but not at all attached to it, or otherwise proved to have belonged to it, a "branch," as the workmen thought, eight feet in length and five inches across. No trace has yet been found of what became of No. 3, or of either it or of No. 4 having been examined by any scientific inquirer.

The succeeding remarks relate cursorily to No. 4, at Barnton House, but chiefly to those now shown in the quarry, and to the so-called "branch."

Mr Witham's fossil of 1830 lay with its lower end downwards, without either branches or roots. The lowest 12 feet are still in excellent preservation in front of the Botanic Garden Herbarium-House; and what appears to be the next 18 feet is in equally good preservation before that part of the Museum of Science and Art now building. The fossil now principally shown in the quarry is somewhat curved, apparently from several fractures occasioned *in situ*. It lies in a west and easterly direction, slightly southward, with the cord of its whole visible length inclined to the horizon at an angle of about 60°, that of the surrounding rock being only 28°. As no record remains of what has been lost of its upper part, and the quarrying has not reached its termination below, its position in relation to that of the living tree cannot be positively settled. Its present top must be 120 feet under the upper surface of the rock of the quarry.* The other (No. 6), of which little more than the cross section is now visible,* seems to lie much in the same direction as

* *June* 30, 1873.—The upper fossil, No. 5, has been pulled down, and is about to be removed to the British Museum. Four feet of the lower one, No. 6, have been conveyed to the Botanic Garden of Edinburgh. One block of the former is 14 feet in girth. The latter, which is rudely cylindrical, measures exactly 8 feet 9 inches in circumference. Its angle of inclination was accurately ascertained before removal to be 61°.

the last, and at a similar inclination of 60° to the horizon, but where the dip of the rocky strata is 38°; and it must have been covered with at least 180 feet of the very hard Craigleith sandstone.

So much of the lowest visible part of No. 5 is uncovered, that its girth may be safely estimated at nearly 10 feet; but at 11 feet higher up it swells out, like some rugged old elms, and must measure considerably more.* The girth of No. 6, in the bottom of the quarry, is very nearly 9 feet. These are much greater trunks than Mr Witham's of 1830, which, at 12 feet from its root, now measures 27 inches by 17. It is considerably flattened along its whole length; but No. 4, at Barnton House, is comparatively little flattened; and those now visible in the quarry seem scarcely flattened at all.

The woody structure of the trees has been more or less perfectly preserved in all of these fossils. There is nothing under this head to alter in the description and delineations by Witham of the fossil found in 1830, and little to add. Many portions present little remains of vegetable structure, and others the appearance of mineral matters only, crystalline, and without any other structural character; but many exhibit in perfection the minute microscopical woody structure represented in Witham's drawings. No. 6, that most recently discovered, shows the woody structure perfect and undeformed, so far as yet examined, from circumference to centre; but it also contains some small cavities containing nodules of pearl-spar; and one considerable cavity has been found, which besides these nodules, has a lining of perfect crystals of calc-spar three-quarters of an inch across. The "branch" shows the woody structure best of all.

This specimen, of which there were originally 8 feet, is now reduced to 18 inches. Its transverse section is rudely semi-oval. Its end is ruggedly pointed. It had been covered all over with a thin coat of coal, of which more will be said presently. Under this the naked eye may see the longitudinal fibrous appearance of wood. A cross section takes on the colour and polish of fine black marble; and on the surface are dimly seen ten lines, marking the boundaries of annual layers, extending from the straight edge right across,

* See Note on p. 105.

from a tenth to half an inch apart, and on the whole parallel to one another. A thin section, made soon after this notice was read, proves that these really are the boundary lines of annual woody layers, the structure of which is beautifully shown before a common lens. Hence the supposed branch is not such, but a longitudinal sector of one, or possibly of a trunk.

All these fossils are covered with a black shining crust of brittle, caking coal, from a tenth to a twentieth of an inch in thickness, easily detached from the surface underneath. This substance, when heated, froths up very much, emits much white dense flame, cakes firmly, and burns slowly away, leaving only from 3·5 to 4·5 per-cent. of ash.

Under this crust the fossils are of a uniform grey colour, like our ordinary tertiary limestone. In most of them the fossilising material is very tough, and hard enough to strike fire with the hammer. No. 6, however, is dark grey. almost black indeed, till it is thoroughly dried, and not so hard as the others. It has evidently been long soaked in the moist bottom of the quarry. In all the mineralising material is essentially the same, and totally different from that of the containing strata. These are all a very pure, compact, fine-grained, extremely hard, siliceous sandstone. Witham has given no fewer than four analyses of his fossils, but they are all either erroneous or incomplete. Silica occurs, not uniformly, in rough particles, to all appearance adventitious, and amounting to about 0·5 per-cent only. Alumina is also present to about the same amount. But the great mass of fossilising matter consists of the carbonates of iron, magnesia, and lime, each in notable proportion. The relative proportion of these carbonates varies, even in different parts of the same fossil, the average being about 60 per-cent. of carbonate of lime, 17 of magnesia, and 14 of carbonate of iron in the form of protoxide. The proportion of the last ingredient varies most of all, for it sometimes rises so high as 28. The most interesting ingredient, however, is carbonaceous matter, which is always left after the solvent action of acids, in a proportion varying from 2·75 to 9·0 per-cent, and in a state of extremely fine division. This is not coal, like the external crust, but charcoal, burning away with a red glow, and no flame, and apparently not possessing the properties of graphitic charcoal. It

sometimes occurs loose in cavities, two of which were found with a considerable loose lining of it.

Nothing has been found on the exterior of these fossils distinctly or even probably referrible to the bark of the original trees. This deficiency is explicable, if, as various circumstances seem to indicate, the trees did not grow where they lie, but have been water-borne, so that their bark, like their roots and branches, had been worn away. The outer crust of coal has been thought to represent the bark in such fossils, but that cannot be here; for, in the first place, it covers large surfaces of the trunk of No. 5, which are evidently the places from which lost branches had sprung; and, secondly, which is more to the point, it uniformly covers the blunt, rugged point, and the complete circuit, of the split sector, which was supposed erroneously by the quarrymen to be a branch entire in its whole circumference, and over the greater part of which it is impossible that there could have been any bark. It is difficult to say how this crust was formed. It is also a very difficult question to settle, how the carbon of this exterior crust was converted into coal, and that of the interior into charcoal. But further examination of such fossils may supply the answer, and throw some light on the process of formation of coal in general.

2. On the Formation of Buds and Roots by the Leaves of the Ipecacuan Plant (*Cephaelis Ipecacuanha*). By Professor Balfour.

The rapid propagation of Ipecacuan in India is an object of importance, and as such has occupied the attention of the Indian Government. The Edinburgh Botanic Garden has contributed largely to the stock of Ipecacuan plants now in cultivation in India. The plan of sending cuttings of the roots or rather rhizomes enveloped in moss has been very successful. We have been able in 1873 to send these cuttings in small boxes through the post. Dr Henderson, the present interim Director of the Botanic Garden at Calcutta, reports most favourably of this plan. He carried out to Calcutta in 1872 small boxes 8 inches by 2, containing germinating rhizomes of Ipecacuan, and roots of Jalap. These are now thriving under his charge. This mode of transmission will save much

trouble and anxiety, and will insure an easy and rapid propagation of the plants. We may expect thus to secure for India a large supply of this invaluable remedy for dysentery.

In reference to the Ipecacuan plants recently sent to India in Wardian cases partly in earth and partly in moss, as well as in boxes with moss, Dr Henderson at Calcutta reports (18th March 1873) that the 120 plants of Ipecacuan taken by him from the Edinburgh Botanic Garden, have increased to 620 in three and a-half months and are all thriving. The smallest possible slice of the rhizome $\frac{1}{18}$th of an inch thick will form a plant. He has scarcely lost a single cutting. Mr Andrew T. Jaffray at Darjeeling, writes on the 19th March to the effect that he has now 7000 plants of Ipecacuan in cultivation.

a

In my communication last session to the Royal Society I stated that Mr Robert Lindsay, foreman in the propagating department of the Botanic Garden, had discovered that the petiole of the leaf of the Ipecacuan plant when put into the soil was capable of producing roots and buds. He has carried out the experiment fully, and I have to report the results. He states that leaves of Ipecacuan plants were removed with the petioles on 27th June 1872. Some were taken off close to the stem, while others were cut off above their attachment to the stem. They were inserted in sandy soil, and placed in a warm moist propagating house, and both gave out roots.

In about three weeks, the end of the petiole where it had been broken off or cut, was cicatrised and formed a rounded pea-like swelling. Shortly afterwards small fibrous roots were produced.

These after some time presented an annulated appearance, as shown in the preceding woodcut. Buds then began to arise from the rounded end of the leaf-stalk. In the woodcut a young shoot is shown arising from the petiole. Mr Lindsay tried to get buds from the leaves by simply placing them flat on the soil (like those of *Bryophyllum* and *Gesnera*), but in this he did not succeed. He found, however, that if the upper part of the leaf was cut off transversely, and the petiole was planted with only the lower half of the lamina attached to it, the growth of roots went on. But the upper half of the leaf when planted did not root. He also ascertained that, by cutting the leaf longitudinally through the midrib, and planting each half, he was able to get roots and buds from each of the halves after the wounds had cicatrised.

These experiments of Mr Lindsay demonstrate the facility with which Ipecacuan may be cultivated, and they supply useful hints to those who are superintending the growth of Ipecacuan.

Fresh specimens were produced, showing the result of Mr Lindsay's experiments, and drawings were exhibited, which had been executed by Mr Francis M. Caird, one of the assistants in the Botanical Class of the University.

EXPLANATION OF WOODCUT.

Ipecacuan Leaf with Petiole, Annulated Root, and young Plant. (From a drawing by Mr F. M. Caird.)

a. Lamina or blade of leaf.
b. Petiole or leaf-stalk.
c. Swelling at the end of the petiole after being placed in the soil.
d. Root proceeding from the swelling, showing an annulated form.
e. Young Plant arising from the swelling of the petiole.

3. On the Physiological Action of Light. No. II. By James Dewar, Esq., and John G. M'Kendrick, M.D.

Since the date of the first communication, we have endeavoured to obtain quantitative results involving time as a variable element in the case of the action of light on the retina and optic nerve. We have therefore found it necessary to construct a true graphical

representation of the variations of the electro-motive force occasioned by the impact and cessation of light. It is clear that to register minute galvanometrical alterations, the only plan that could be employed would be to photograph on a sensitive surface, covering a cylinder rapidly revolving on a horizontal axis, the alteration of position of the spot of light reflected from the mirror, just as continuous magnetic observations are registered. As the apparatus required to execute these observations is very complicated, and would require much preliminary practice, we have in the meantime adopted a simpler method of registration. This plan is to note the position of the galvanometer at equal intervals of time, before, during, and after, the impact of light on the eye. In these observations we have used a seconds' pendulum giving a loud beat. One observer reads aloud the galvanometer, the other marks every interval of two and a half seconds, registers the numbers obtained, and regulates the supply of light. A little practice in the method above described has enabled us to obtain very satisfactory results, agreeing very closely in different observations, and showing in a decided way the salient points of the variation curve.

These curves show, that on the impact of light there is a sudden increase of the electro-motive force; during the continuance of light it falls to a minimum value; and on the withdrawal of light there is what we term an *inductive effect*, that is to say, a sudden increase of the electro-motive force which enables the nerve to acquire its normal energy. The falling-off of the electro-motive force by the continued action of light is the physical representative of what, in physiological language, is called fatigue; the inductive effect exhibiting the return of the structure to its normal state.

Occasionally the impact of light is not followed by a rise in the electro-motive force, but by a diminution. This is probably to be explained by the fact, that the death of the retina and nerve is indicated by a gradual falling of the electro-motive force, and that this change frequently goes on so rapidly that the impact of light is unable to produce any rise. In these circumstances, the spot of light, which before the impact of light was slowly moving downwards, is on the impact steadied for a moment, and then pursues its downward course more rapidly.

We have carried out, since last communication, several distinct sets of observations:—

1. We have proved that though there is no difficulty in obtaining a strong current from the skin of the frog, this current is not affected by light. This observation demonstrates that the pigment cells of the skin in the vicinity of the cornea have nothing to do with the results obtained.

2. The current obtained from a mass of the pigment cells of the choroid does not exhibit any sensitiveness to light.

3. The subcutaneous injection into the frog of Woorara, Santonin, Belladonna, and Calabar bean, does not destroy the sensibility of the retina to light.

4. As to the action of the anterior portion of the eye. On carefully bisecting an eye of a frog, so as to remove completely the anterior portion, including cornea, aqueous humour, iris, ciliary muscle, and lens, and on bringing the retina into actual contact with one of the clay pads, we readily obtained a large deflection, which was as sensitive to light as when the whole eye was employed, thus eliminating any possibility of the contraction of the iris under the stimulus of light having to do with the results previously obtained.

5. On using the anterior portion of the eye so that the cornea and posterior surface of the crystalline lens were the poles, we obtained a large deflection, which was, however, insensible to light.

6. The sclerotic and nerve without the retina, in the same manner, gave a large natural electro-motive force, also not sensitive.

7. The distribution of the electro-motive force between the different portions of the eye and cross section of the nerve may be stated as follows: The most positive structure is the cornea, then the sclerotic, then the longitudinal surface of the nerve; the cornea is also positive to the posterior surface of the crystalline lens, and the retina itself seems to be positive to the transverse section of the nerve.

8. As to *the effects produced by lights of different intensities.*—If a candle is placed at a distance of one foot from the eye, and then is removed ten feet, the amount of light received by the eye is exactly one hundredth part of what it got at a distance of one foot; whereas the electro-motive force, instead of being altered in the same pro-

portion, is only reduced to one-third. Repeated experiments made with the eye in different positions have conclusively shown that a quantity of light one hundred times in excess of another quantity only modifies the electro-motive force to the extent of increasing it three times as much, certainly not more.

9. It was apparent to us that these experiments would ultimately bear upon the theory of sense-perception as connected with vision. It is now generally admitted that no image, as such, of an external object, is conveyed to the sensorium, but that in reality the brain receives certain impressions of alterations taking place in the receiving organ. The natural query then arises—are the physical effects we have described and measured really comparable in any way with our sensational differences in light perception when we eliminate all mental processes of association, &c., and leave only perception of difference of intensity? In other words, are these changes the representative of what is conveyed to the sensorium? It would appear, at first sight, that this problem is altogether beyond experimental inquiry. There is, however, a way of arriving at very accurate measures of the variation of our sensational differences in the case of light, and this has been developed theoretically and experimentally by the justly renowned physiologist Fechner. Stating the law of Fechner * generally, we may say, the difference of our sensations is proportional to the logarithm of the quotient of the respective luminous intensities. A recent series of experiments by Dalbœuf † has entirely confirmed the truth of this law. If, therefore, the observed differences in electro-motive power, registered under conditions of varying luminous intensity, agree with this law of Fechner, regulating our sensational impressions, then there can be little doubt these variations are the cause of, and are comparable to, our perception of sensational differences. Now, we have stated above, that with a quantity of light one hundred times in excess of another quantity, the electro-motive force only becomes three times greater. According to Fechner's law, we may say the difference of our sensations, with that variation in the amount of luminous intensity, would be represented by 2, the logarithm of 100. Our experimental results being as 3 to 1, the difference is

* Fechner, *Elemente der Psychophysik*. Helmholtz, *Optique Physiologique*.

† Recent Memoir to Belgian Academy.

also 2, thus agreeing very closely. It is to be remembered, however, that these results have been obtained by experiment on the eye of the frog, but similar changes have been observed in the eyes of mammals. In the latter, however, the amount of alteration is not so great, in all probability owing to the rapid death of the parts.

10. When one clay-point is placed in contact with the cornea or nerve, and the other with the section of the optic lobe, a current is at once obtained, which is sensitive to light. In this experiment the eye is left in the orbit, and the nerve is uninjured. Thus, the effect of light on the retina has been traced into the brain.

The following Gentlemen were elected Fellows of the Society:—

Donald Crawford, M.A., Advocate, Fellow of Lincoln College, Oxford.
M. M. Pattison Muir, Esq., Senior Assistant in the Andersonian Laboratory, Glasgow.

Monday, 19th May 1873.

D. MILNE HOME, LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. On the Thermal Influence of Forests. By Robert Louis Stevenson, Esq. Communicated by Thomas Stevenson, Esq.

The opportunity of an experiment on a comparatively large scale, and under conditions of comparative isolation, can occur but rarely in such a science as Meteorology. Hence Mr Milne Home's proposal for the plantation of Malta seemed to offer an exceptional opportunity for progress. Many of the conditions are favourable to the simplicity of the result; and it seemed natural that, if a searching and systematic series of observations were to be immediately set afoot, and continued during the course of the plantation and the growth of the wood, some light would be thrown on the still doubtful question of the climatic influence of forests.

Mr Milne Home expects, as I gather, a threefold result:—1st, an increased and better regulated supply of available water; 2d, an increased rainfall; and, 3d, a more equable climate, with more

temperate summer heat and winter cold.* As to the first of these expectations, I suppose there can be no doubt that it is justified by facts; but it may not be unnecessary to guard against any confusion of the first with the second. Not only does the presence of growing timber increase and regulate the supply of running and spring water independently of any change in the amount of rainfall, but, as Boussingault found at Marmato,† denudation of forest is sufficient to decrease that supply, even when the rainfall has increased instead of diminished in amount. The second and third effects stand apart, therefore, from any question as to the utility of Mr Milne Home's important proposal; they are both, perhaps, worthy of discussion at the present time, but I wish to confine myself in the present paper to the examination of the third alone.

A wood, then, may be regarded either as a *superficies* or as a *solid;* that is, either as a part of the earth's surface slightly elevated above the rest, or as a diffused and heterogeneous body displacing a certain portion of free and mobile atmosphere. It is primarily in the first character that it attracts our attention, as a radiating and absorbing surface, exposed to the sun and the currents of the air; such that, if we imagine a plateau of meadow-land or bare earth raised to the mean level of the forest's exposed leaf-surface, we shall have an agent entirely similar in kind, although perhaps widely differing in the amount of action. Now, by comparing a tract of wood with such a plateau as we have just supposed, we shall arrive at a clear idea of the specialties of the former. In the first place, then, the mass of foliage may be expected to increase the radiating power of each tree. The upper leaves radiate freely towards the stars and the cold inter-stellar spaces, while the lower ones radiate to those above and receive less heat in return; consequently, during the absence of the sun, each tree cools gradually downward from top to bottom. Hence we must take into account not merely the area of leaf-surface actually exposed to the sky, but, to a greater or less extent, the surface of every leaf in the whole tree or the whole wood. This is evidently a point in which the action of the forest may be expected to differ from that of the meadow or naked earth; for though, of course, inferior strata tend

* Journal Scot. Met. Soc., New Series, No. xxvi., p. 35.

† Quoted by Mr Milne Home.

to a certain extent to follow somewhat the same course as the mass of inferior leaves, they do so to a less degree—conduction, and the conduction of a very slow conductor, being substituted for radiation.

We come next, however, to a second point of difference. In the case of the meadow, the chilled air continues to lie upon the surface, the grass, as Humboldt says, remaining all night submerged in the stratum of lowest temperature; while in the case of trees, the coldest air is continually passing down to the space underneath the boughs, or what we may perhaps term the crypt of the forest. Here it is that the consideration of any piece of woodland conceived as a solid comes naturally in; for this solid contains a portion of the atmosphere, partially cut off from the rest, more or less excluded from the influence of wind, and lying upon a soil that is screened all day from insolation by the impending mass of foliage. In this way (and chiefly, I think, from the exclusion of winds), we have underneath the radiating leaf-surface a stratum of comparatively stagnant air, protected from many sudden variations of temperature, and tending only slowly to bring itself into equilibrium with the more general changes that take place in the free atmosphere.

Over and above what has been mentioned, thermal effects have been attributed to the vital activity of the leaves in the transudation of water, and even to the respiration and circulation of living wood. The whole actual amount of thermal influence, however, is so small that I may rest satisfied with mere mention. If these actions have any effect at all, it must be practically insensible; and the others that I have already stated are not only sufficient validly to account for all the observed differences, but would lead naturally to the expectation of differences very much larger and better marked. To these observations I proceed at once. Experience has been acquired upon the following three points:—1. The relation between the temperature of the trunk of a tree and the temperature of the surrounding atmosphere; 2. The relation between the temperature of the air under a wood and the temperature of the air outside; and, 3. The relation between the temperature of the air above a wood and the temperature of the air above cleared land.

As to the first question, there are several independent series of

observations; and I may remark in passing, what applies to all, that allowance must be made throughout for some factor of specific heat. The results were as follows:—The seasonal and monthly means in the tree and in the air were not sensibly different. The variations in the tree, in M. Becquerel's own observations, appear as considerably less than a fourth of those in the atmosphere, and he has calculated, from observations made at Geneva between 1796 and 1798, that the variations in the tree were less than a fifth of those in the air; but the tree in this case, besides being of a different species, was seven or eight inches thicker than the one experimented on by himself.* The variations in the tree, therefore, are always less than those in the air, the ratio between the two depending apparently on the thickness of the tree in question and the rapidity with which the variations followed upon one another. The times of the maxima, moreover, were widely different: in the air, the maximum occurs at 2 P.M. in winter, and at 3 P.M. in summer; in the tree, it occurs in winter at 6 P.M., and in summer between 10 and 11 P.M. At nine in the morning in the month of June, the temperatures of the tree and of the air had come to an equilibrium. A similar difference of progression is visible in the means, which differ most in spring and autumn, and tend to equalise themselves in winter and in summer. But it appears most strikingly in the case of variations somewhat longer in period than the daily ranges. The following temperatures occurred during M. Becquerel's observations in the Jardin des Plantes:—

1859.

Date.	Temperature of the Air.	Temperature in the Tree.
	°	°
Dec. 15,	26·78	32
„ 16,	19.76	32
„ 17,	17·78	31·46
„ 18,	13·28	30·56
„ 19,	12·02	28·40
„ 20,	12·54	25·34
„ 21,	38·30	27·86
„ 22,	43·34	30·92
„ 23,	44·06	31·46

* Atlas Meteorologique de l'Observatoire Imperial, 1867.

A moment's comparison of the two columns will make the principle apparent. The temperature of the air falls nearly fifteen degrees in five days; the temperature of the tree, sluggishly following, falls in the same time less than four degrees. Between the 19th and the 20th the temperature of the air has changed its direction of motion, and risen nearly a degree; but the temperature of the tree persists in its former course, and continues to fall nearly three degrees farther. On the 21st there comes a sudden increase of heat, a sudden thaw; the temperature of the air rises twenty-five and a-half degrees; the change at last reaches the tree, but only raises its temperature by less than three degrees; and even two days afterwards, when the air is already twelve degrees above freezing point, the tree is still half a degree below it. Take, again, the following case:—

1859.

Date.	Temperature of the Air.	Temperature in the Tree.
	°	°
July 13,	84·92	76·28
„ 14,	82·58	78·62
„ 15,	80·42	77·72
„ 16,	79·88	78·44
„ 17,	73·22	75·92
„ 18,	68·54	74·30
„ 19,	65·66	70·70

The same order reappears. From the 13th to the 19th the temperature of the air steadily falls, while the temperature of the tree continues apparently to follow the course of previous variations, and does not really begin to fall, is not really affected by the ebb of heat, until the 17th, three days at least after it had been operating in the air.* Hence we may conclude that all variations of the temperature of the air, whatever be their period, from twenty-four hours up to twelve months, are followed in the same manner by variations in the temperature of the tree; and that those in the tree are always less in amount and considerably slower of occurrence than those in the air. This *thermal sluggishness*, so to speak, seems capable of explaining all the phenomena of the case without

* Comptes Rendus de l'Academie, 29th March 1869.

any hypothetical vital power of resisting temperatures below the freezing point, such as is hinted at even by Becquerel.

Reaumur, indeed, is said to have observed temperatures in slender trees nearly thirty degrees higher than the temperature of the air in the sun; but we are not informed as to the conditions under which this observation was made, and it is therefore impossible to assign to it its proper value. The sap of the ice-plant is said to be materially colder than the surrounding atmosphere; and there are several other somewhat incongruous facts, which tend, at first sight, to favour the view of some inherent power of resistance in some plants to high temperatures, and in others to low temperatures.* But such a supposition seems in the meantime to be gratuitous. Keeping in view the thermal redispositions, which must be greatly favoured by the ascent of the sap, and the difference between the condition as to temperature of such parts as the root, the heart of the trunk, and the extreme foliage, and never forgetting the unknown factor of specific heat, we may still regard it as possible to account for all anomalies without the aid of any such hypothesis. We may, therefore, I think, disregard small exceptions, and state the result as follows:—

If, after every rise or fall, the temperature of the air remained stationary for a length of time proportional to the amount of the change, it seems probable—setting aside all question of vital heat—that the temperature of the tree would always finally equalise itself with the new temperature of the air, and that the range in tree and atmosphere would thus become the same. This pause, however, does not occur: the variations follow each other without interval; and the slow-conducting wood is never allowed enough time to overtake the rapid changes of the more sensitive air. Hence, so far as we can see at present, trees appear to be simply bad conductors, and to have no more influence upon the temperature of their surroundings than is fully accounted for by the consequent tardiness of their thermal variations.

Observations bearing on the second of the three points have been made by Becquerel in France, by La Cour in Jutland and Iceland, and by Rivoli at Posen. The results are perfectly con-

* Prof. Balfour's Class-Book of Botany, Physiology, chap. xii. page 670.

gruous. Becquerel's observations * were made under wood, and about a hundred yards outside in open ground, at three stations in the district of Montargis, Loiret. There was a difference of more than one degree Fahrenheit between the mean annual temperatures in favour of the open ground. The mean summer temperature in the wood was from two to three degrees lower than the mean summer temperature outside. The mean maxima in the wood were also lower than those without by a little more than two degrees. Herr La Cour † found the daily range consistently smaller inside the wood than outside. As far as regards the mean winter temperatures, there is an excess in favour of the forest, but so trifling in amount as to be unworthy of much consideration. Libri found that the minimum winter temperatures were not sensibly lower at Florence, after the Appenines had been denuded of forest, than they had been before.‡ The disheartening contradictoriness of his observations on this subject led Herr Rivoli to the following ingenious and satisfactory comparison.§ Arranging his results according to the wind that blew on the day of observation, he set against each other the variation of the temperature under wood from that without, and the variation of the temperature of the wind from the local mean for the month:—

Wind, . .	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
Var. in Wood,	+0·60	+0·26	+0·26	+0·04	−0·04	−0·20	+0·16	+0·07
Var. in Wind,	−0·30	−2·60	−3·30	−1·20	+1·00	+1·30	+1·00	+1·00

From this curious comparison, it becomes apparent that the variations of the difference in question depend upon the amount of variations of temperature which take place in the free air, and on the slowness with which such changes are communicated to the stagnant atmosphere of woods; in other words, as Herr Rivoli boldly formulates it, a forest is simply a bad conductor. But this

* Comptes Rendus, 1867 and 1869. † See his paper.

‡ Annales de Chimie et de Physique, xlv., 1830. A more detailed comparison of the climates in question would be a most interesting and important contribution to the subject.

§ Reviewed in the Austrian Meteorological Magazine, vol. iv. p. 548.

is precisely the same conclusion as we have already arrived at with regard to individual trees; and in Herr Rivoli's table, what we see is just another case of what we saw in M. Becquerel's—the different progression of temperatures. It must be obvious, however, that the thermal condition of a single tree must be different in many ways from that of a combination of trees and more or less stagnant air, such as we call a forest. And accordingly we find, in the case of the latter, the following new feature: The mean yearly temperature of woods is lower than the mean yearly temperature of free air, while they are decidedly colder in summer, and very little, if at all, warmer in winter. Hence, on the whole, forests are colder than cleared lands. But this is just what might have been expected from the amount of evaporation, the continued descent of cold air, and its stagnation in the close and sunless crypt of a forest; and one can only wonder here, as elsewhere, that the resultant difference is so insignificant and doubtful.

We come now to the third point in question, the thermal influence of woods upon the air above them. It will be remembered that we have seen reason to believe their effect to be similar to that of certain other surfaces, except in so far as it may be altered, in the case of the forest, by the greater extent of effective radiating area, and by the possibility of generating a descending cold current as well as an ascending hot one. M. Becquerel is (so far as I can learn) the only observer who has taken up the elucidation of this subject. He placed his thermometers at three points: * A and B were both about seventy feet above the surface of the ground; but A was at the summit of a chestnut tree, while B was in the free air, fifty feet away from the other. C was four or five feet above the ground, with a northern exposure; there was also a fourth station to the south, at the same level as this last, but its readings are very seldom referred to. After several years of observation, the mean temperature at A was found to be between one and two degrees higher than that at B. The order of progression of differences is as instructive here as in the two former investigations. The maximum difference in favour of station A occurred between three and five in the afternoon, later or sooner according as there

* Comptes Rendus, 28th May 1860.

had been more or less sunshine, and ranged sometimes as high as seven degrees. After this the difference kept declining until sunrise, when there was often a difference of a degree, or a degree and a half, upon the other side. On cloudy days the difference tended to a minimum. During a rainy month of April, for example, the difference in favour of station A was less than half a degree; the first fifteen days of May following, however, were sunny, and the difference rose to more than a degree and a half.* It will be observed that I have omitted up to the present point all mention of station C. I do so because M. Becquerel's language leaves it doubtful whether the observations made at this station are logically comparable with those made at the other two. If the end in view were to compare the progression of temperatures above the earth, above a tree, and in free air, removed from all such radiative and absorptive influences, it is plain that all three should have been equally exposed to the sun or kept equally in shadow. As the observations were made, they give us no notion of the relative action of earth-surface and forest-surface upon the temperature of the contiguous atmosphere; and this, as it seems to me, was just the *crux* of the problem. So far, however, as they go, they seem to justify the view that all these actions are the same in kind, however they may differ in degree. We find the forest heating the air during the day, and heating it more or less according as there has been more or less sunshine for it to absorb, and we find it also chilling it during the night; both of which are actions common to any radiating surface, and would be produced, if with differences of amount and time, by any other such surface raised to the mean level of the exposed foliage.

To recapitulate:

1st, We find that single trees appear to act simply as bad conductors.

2d, We find that woods, regarded as solids, are, on the whole, slightly lower in temperature than the free air which they have displaced, and that they tend slowly to adapt themselves to the various thermal changes that take place without them.

3d, We find forests regarded as surfaces acting like any other

* Comptes Rendus, 20th May 1861.

part of the earth's surface, probably with more or less difference in amount and progression, which we still lack the information necessary to estimate.

All this done, I am afraid that there can be little doubt that the more general climatic investigations will be long and vexatious. Even in South America, with extremely favourable conditions, the result is far from being definite. Glancing over the table published by M. Becquerel in his book on climates, from the observations of Humboldt, Hall, Boussingault, and others, it becomes evident, I think, that nothing can be founded upon the comparisons therein instituted; that all reasoning, in the present state of our information, is premature and unreliable. Strong statements have certainly been made; and particular cases lend themselves to the formation of hasty judgments. "From the Bay of Cupica to the Gulf of Guayaquil," says M. Boussingault, "the country is covered with immense forests and traversed by numerous rivers; it rains there almost ceaselessly; and the mean temperature of this moist district scarcely reaches 78°·8 F. . . . . . At Payta commence the sandy deserts of Priura and Sechura; to the constant humidity of Choco succeeds almost at once an extreme of dryness; and the mean temperature of the coast increases at the same time by 1°·8 F." * Even in this selected favourable instance it might be argued that the part performed in the change by the presence or absence of forest was comparatively small; there seems to have been, at the same time, an entire change of soil; and, in our present ignorance, it would be difficult to say by how much this of itself is able to affect the climate. Moreover, it is possible that the humidity of the one district is due to other causes besides the presence of wood, or even that the presence of wood is itself only an effect of some more general difference or combination of differences. Be that as it may, however, we have only to look a little longer at the table before referred to, to see how little weight can be laid on such special instances. Let us take five stations, all in this very district of Choco. Hacquita is eight hundred and twenty feet above Novita, and their mean temperatures are the same. Alto de Mombu, again, is five hundred

* Becquerel, "Climats," p. 141.

feet higher than Hacquita, and the mean temperature has here fallen nearly two degrees. Go up another five hundred feet to Tambo de la Orquita, and again we find no fall in the mean temperature. Go up some five hundred further to Chami, and there is a fall in the mean temperature of nearly six degrees. Such numbers are evidently quite untrustworthy; and hence we may judge how much confidence can be placed in any generalisation from these South American mean temperatures.

The question is probably considered too simply—too much to the neglect of concurrent influences. Until we know, for example, somewhat more of the comparative radiant powers of different soils, we cannot expect any very definite result. A change of temperature would certainly be effected by the plantation of such a marshy district as the Sologne, because, if nothing else were done, the roots might pierce the impenetrable subsoil, allow the surface-water to drain itself off, and thus dry the country. But might not the change be quite different if the soil planted were a shifting sand, which, *fixed* by the roots of the trees, would become gradually covered with a vegetable earth, and thus be changed from dry to wet? Again, the complication and conflict of effects arises, not only from the soil, vegetation, and geographical position of the place of the experiment itself, but from the distribution of similar or different conditions in its immediate neighbourhood, and probably to great distances on every side. A forest, for example, as we know from Herr Rivoli's comparison, would exercise a perfectly different influence in a cold country subject to warm winds, and in a warm country subject to cold winds; so that our question might meet with different solutions even on the east and west coasts of Great Britain.

The consideration of such a complexity points more and more to the plantation of Malta as an occasion of special importance; its insular position and the unity of its geological structure both tend to simplify the question. There are certain points about the existing climate, moreover, which seem specially calculated to throw the influence of woods into a strong relief. Thus, during four summer months, there is practically no rainfall. Thus, again, the northerly winds when stormy, and especially in winter, tend to depress the temperature very suddenly; and thus, too, the southerly

and south-westerly winds, which raise the temperature during their prevalence to from eighty-eight to ninety-eight degrees, seldom last longer than a few hours; insomuch that "their disagreeable heat and dryness may be escaped by carefully closing the windows and doors of apartments at their onset."* Such sudden and short variations seem just what is wanted to accentuate the differences in question. Accordingly, the opportunity seems one not lightly to be lost, and the British Association or this Society itself might take the matter up and establish a series of observations, to be continued during the next few years. Such a combination of favourable circumstances may not occur again for years; and when the whole subject is at a stand-still for want of facts, the present occasion ought not to go past unimproved.

Such observations might include the following:—

The observation of maximum and minimum thermometers in three different classes of situation—*videlicet*, in the areas selected for plantation themselves, at places in the immediate neighbourhood of those areas where the external influence might be expected to reach its maximum, and at places distant from those areas where the influence might be expected to be least.

The observation of rain-gauges and hygrometers at the same three descriptions of locality.

In addition to the ordinary hours of observation, special readings of the thermometers should be made as often as possible at a change of wind and throughout the course of the short hot breezes alluded to already, in order to admit of the recognition and extension of Herr Rivoli's comparison.

Observation of the periods and forces of the land and sea breezes.

Gauging of the principal springs, both in the neighbourhood of the areas of plantation and at places far removed from those areas.

* Scoresby-Jackson's "Medical Climatology."

2. Observations and Experiments on the Fluid in the Cavities of Calcareous Spar. By Dr James Hunter and Edward Sang.

At a recent meeting I laid before the Society a short notice of a phenomenon exhibited by the fluid contained in the cavities of calcareous spar. This phenomenon had been observed only a few days before, and the notice was given for the purpose of directing to it the attention of other observers, and particularly of those who happen to possess other minerals with analogous cavities, and I now propose to give an account of some more recent experiments and observations in regard to it.

Of all known minerals, carbonate of lime presents the greatest facility for the study of the laws of crystallisation. We trace in it evidences of the stoppage and resumption of growth; we see marks of abrasion and fracture on surfaces once external but now covered over; layers of mud and portions of extraneous bodies are seen inclosed; yet amid all of these interruptions the direction of the planes of crystallisation are kept with remarkable persistence.

The ultimate or outer surface of a piece of Iceland spar of any great size presents a rough appearance, caused by the meeting of many surfaces of, as it were, smaller crystals; the hollows among these had not been filled up when the deposition ceased. If now there be an accession of liquid holding lime in solution, and the crystallisation be renewed, these hollows may not be filled up from the bottom, but may be covered over by the new mineral, leaving the spaces full of the mother liquid, so that when the whole mass has been cooled a small vacuity is left. Sometimes these cavities are very irregular, at other times their surfaces are beautifully flat and often obviously parallel to the cleavage planes of the spar. Hence, in mounting such specimens for microscopic observation, we must be careful not to heat, or at least not to overheat, the spar; cold cement is at all times preferable.

On looking at any object in the interior of a piece of spar we see two images, one belonging to the ordinarily, the other to the extraordinarily refracted light. Now, in all bits of spar containing faults, the crystallisation has been interrupted and carried on in

various conditions as to temperature; hence the direction of the axis of crystallisation is not absolutely kept, as is obvious on any of the cleavage surfaces. Hence the path of the extraordinarily refracted ray is devious, and the image blurred; while the path of the ordinary ray, depending only on the homogeneity of the substance, is straight. For the purpose, therefore, of viewing anything in the interior it is proper to eliminate the extraordinary light by using a polarising reflector, a Nicol's prism, or something equivalent. This blurredness of the extraordinary image is common in crystals of other substances, and is due to the very same cause.

When a piece of spar containing a flat-faced cavity is placed under the microscope, and a small coin or other bit of metal is brought near it, the fluid is observed to take the opposite end of the cavity. For convenience a type-space was mounted on the end of a wire fixed to a stand so as to be readily brought into position, and the same repulsion was observed; here it seemed obvious that the metal and the spar had both the same temperature with the room, and thus there was no ground for suspicion that temperature had to do with the phenomenon.

Dr James Hunter, while repeating the trials, observed that a coin freshly laid down acted well, but that after some time its repulsion was less; he observed the same thing of a recently rubbed coin. This led him to suspect the agency of heat, and on repeating his trials it became clear that a difference of temperature is essential to the exhibition of this repulsion. He also found that any substance when warmed possesses the same property, and lost no time in communicating to me the result of his observations. This led Mr E. Elmslie Sang to suggest the trial of metal cooled below the temperature of the room; and, on returning home from the Society's last meeting, I found that Dr Hunter and my son had completed a set of trials showing most clearly that the fluid in the cavity moves from a warmer and toward a colder body. This may be very well shown by placing a piece of metal heated in the hand upon the spar, and so sending the fluid to the farther end. On now wetting the metal with ether, so as to cool it, the fluid is seen to come to the nearer end of the cavity.

This discovery by Dr Hunter completely changed the line of

research in which I was engaged, and rendered any quantitative experiment excessively difficult, because we have no means of determining the temperatures of such small masses, and because a very slight difference of temperature is enough to produce the observed effect.

The statement that when A is warmer than B we have repulsion, but that when A is the colder we have attraction, cannot be universal, because if A and B were merely to change names the enunciation of the law would be reversed. Such a law can only hold good between members of two distinct classes, and, so far as we have yet seen, this distinction is between solids and fluids. Reflection on this matter brought to my mind a phenomenon with which I have been familiar for more than half a century, and which I used to refer to some peculiar variety of what is called *capillary action.* In preparing a small drill, such as is used by watchmakers, the little tool is first hardened by being plunged while red hot into cold water, and is then *tempered* or softened to the proper degree. This tempering is done by dipping the drill in oil or tallow, and then heating the stock end of it in a small flame. The oil is seen to gather in a drop, which moves rapidly towards the point, and the ebullition of this drop serves to mark the proper temperature.

If we coat a common smooth knitting needle with a film of oil so thin that it will not flow, and, holding the needle horizontally, bring the middle of it to the edge of a flame, we shall see a bulging mass of oil form on each side and move away from the flame, gathering bulk as it proceeds. Here we have a variation of the very phenomenon seen in the calc-spar; the fluid is repelled by the hotter metal. The experiment may be varied thus. Having placed the middle of a cleaned wire in the flame, put a small drop of oil on it near to the flame; this drop will be seen to move towards the colder part of the wire. Another variation is to prepare a thin metallic plate, and to coat its upper surface with a film of oil; when the middle of the plate is set upon a piece of hot iron, the oil gathers in a wave all round the hot part, and slowly recedes from it.

After having assisted at these experiments with the oil, Dr Hunter made a very beautiful variation, which consisted in direct-

ing a stream of warm air upon the end of the piece of spar under the microscope. The fluid recedes from that end, and the action is reversed by changing the position of the current.

On holding horizontally a glass tube, of which the inside has been thinly coated with coloured oil, and on heating a part of it, the oil is seen to leave the heated part and become heaped up on each side. The same thing takes place with water; but on making trial with sulphuric acid, no such effect was perceptible.

The occurrence of so many analogous phenomena points to some general or, at least, comprehensive law; and the question arises—Whether is this motion of the fluid dependent on actual contact, and due to the unequal heating of the adjacent solid, or is it a true repulsion between the colder fluid and the warmer solid, independent altogether of contact?

The instantaneous movement of the fluid when a warm body is brought near to without touching the spar, favours the latter interpretation of the phenomena, but the former interpretation seems to be more in accordance with the other variations of the experiments. If, when a warm body is brought near, the action be to induce an unequal heating of the containing vessel, and if the motions be due to the attractions or repulsions between the fluid and the spar, no real repulsion will be shown between the warm body and the total mass. But if the motion be due to a repulsion between the warm body and the colder fluid, the mass, as a whole, will be repelled. Hence, by poising the vessel containing the fluid so delicately as to allow of this repulsion being exhibited if it exist, we shall be able to determine the true nature of the action. In making the arrangements we must eliminate the influence of aërial currents caused by the difference of temperature. I am in hopes of being able soon to decide the question as between the two interpretations by help of an instrument of sufficient delicacy.

In making the experiments with glass tubes, it was noticed incidentally that when the glass has been so heated as to drive the oil or the water completely from it, the surface has acquired the property of not being easily oiled or wet again. I show one tube, over the surface of which the oil flowed easily; it has been hermetically closed, and has since that been heated. The oil now

refuses to flow, and remains aggregated in oil-drops over the surface. When the glass in the proximity of one of these drops is heated, the oil is seen to creep away from the heated part, leaving behind it no trace of oil on the surface.

3. On "Tait's Property of the Retina." By George Forbes, Esq.

Professor Tait having asked me to communicate to the Society some experiments I have made from time to time on the property of the retina discovered by him, and communicated to this Society, 15th January 1872, I prepared the following notes. It will be remembered that he pointed out that when the eye has been rested for a long time the first impression of light gives a red colour. Professor Crum Brown stated at the same meeting, that after Professor Tait had told him of the appearance he had himself observed a like phenomenon. Awaking one morning at grey dawn, and opening his eyes suddenly, he saw a glare of red on the window, and was so struck by it that he hastily rose to discover what house was on fire.

The circumstances under which Professor Tait made the observation were as follows:—He was suffering from sleepless nights owing to the illness arising from re-vaccination. He found that at each time of awaking, a portion of the wall feebly illuminated by a gas-flame appeared to have a crimson hue, and acquired its true white colour only after a few seconds of time.

I have very little to tell the Society, except to corroborate the evidence of Professor Tait, and to describe a method of observation that removes the necessity for re-vaccination or even sleepless nights. I have reproduced the appearance, I suppose, thirty times during the past winter. I lower the gas until there is only a small blue flame. This may be done before going to bed, and the experiment made in the morning, provided the window is darkened by shutters. In the morning, on suddenly turning up the gas, either the gas-flame assumes the crimson flush, or if there be a globe of ground glass on the gas, that globe assumes the hue. If the gas be quickly lowered again, a short rest is sufficient before repeating the experiment. It is never necessary (in my case) to

be in the dark for more than an hour or so. But when the time of darkness is short, the crimson flush is seen only for a small fraction of a second. It is not necessary to have just awoken from sleep, though certainly this seems to favour the appearance, making it more extended and more lasting. The colour of this appearance is the same as that crimson flush which is often seen when the eyelids are closed and a light is shining on them. This struck both Professor Tait and myself, and led him to test whether it was due to the same cause, viz., the passage of light through the blood-vessels.

* * * * * *

I had proceeded thus far, and had moreover duly apologised to the Society for offering them a communication with so little novelty in it, when accident, or rather an inexcusable drowsiness, led me to perform some experiments that I look upon as of far greater importance, in that they give an extension to the property of the retina observed by Tait, in a direction quite unlooked for.

When travelling in the train from Edinburgh to London lately, I had my eyes closed, and frequently saw that crimson flush which is so often seen under such circumstances, and to which I have already alluded. This has been stated by Professor Tait and myself to be of the same hue as that observed by him in the cases mentioned in his note. It has always been attributed without any doubt to the passage of the light through the blood-vessels of the eyelid. But I soon noticed a remarkable fact, viz., that if the light of the sky remained of the same brightness, in other words, if the sun were not flitting behind clouds, this crimson flush gave place to a dingy orange or even yellowish brown colour. The brilliant crimson flush was in these circumstances seldom visible on closing the eyelids, and it invariably gave way to this dingy colour. On continuing to repeat this experiment, no doubt remained on my mind of the fact. Being now convinced that the appearance of white light passing through the blood-vessels of the eyelid is of this orange colour, I was at a loss to account for the crimson flush that is so often seen. I soon noticed, however, that when the eyes were closed, this brilliant colour never made its appearance, except at such moments as when the sun burst out from behind a cloud, thus brightening the field of view. I then

covered my closed eyes with my hand, so as to cause complete darkness. If I now removed my hand, the eyelids still being closed the crimson flush made its appearance; the darkness having been continued for a considerable time. I soon found that if the closed eyes were first directed to a white handkerchief, and then to the bright sky, the crimson flush made its appearance. At this stage the true explanation of the phenomenon began to appear. It was that the colour of white light that has passed through the eyelid is dingy orange or yellowish brown, and that the crimson flush is due to Tait's property of the retina, namely, that *when the eye is suddenly illuminated, or when the illumination is suddenly increased, the retina first acquires the power of recognising the deep red; but the other colours usually follow so rapidly as to prevent this fact from being recognised.* I hope that Professor Tait will allow me to make this slight addition to his statement, as originally made.

According to this theory, the reason why this flush is only sometimes seen is, that peculiarly favourable circumstances are necessary for observing it. These are (1), a very long rest to the eye, (as this is how Professor Tait and Professor Crum Brown saw it); or (2), a very sudden illumination of the retina (this is the experiment of the gas-flame described in the first part of this communication); or (3), an exposure to a very feeble light after the eye has been in the dark for a short time (this is what I have just described). To prove still further that this, and not the transmission of light through blood, is the true explanation of the crimson flush as usually seen, I tried the following experiment:—A piece of common whitey-brown paper, four folds thick, was placed in front of one eye (the other being quite darkened). This shaded eye was kept dark for a short time, then keeping it closed to the skin to prevent stray light from entering, the head was raised, and the eye opened pointing to the sky. The crimson flush was unprecedentedly vivid, but soon yielded to the yellow colour of the paper employed. Lastly, six folds of plain white glazed writing paper were placed in front of the eye in the same manner. A longer duration of darkness was necessary than in the last case, but then the crimson flush was well shown, the colour then changed to orange, and it was some time before it assumed its natural white colour.

These experiments, then, prove that the transmission of light through the blood-vessels is not necessary for the production of the crimson flush, and that a long rest be given to the eye to perceive the phenomenon described by Professor Tait, and that the former depends upon the latter effect.

In the experiments last described the whole of the retina was affected. There is still one point that requires explanation. How is it that either a very powerful or a very feeble light is the most potent, either a gas flame or diffuse light that has passed through several folds of paper? At first this seems to militate against the identity of the two phenomena, but a little consideration explains difficulty. First, if the light be very bright, *e.g.*, a gas flame, the red will certainly have a greater tendency to appear, but it seems *a priori* likely that the other colours will also soon become apparent. Thus we should expect with a powerful flame to see a very intense redness, lasting a very short time. Second, if the light be very feeble, *e.g.*, diffuse light passing through paper. Here it is not likely that we should get so brilliant a red, but it is certainly very probable that it will be much longer before the other colours become sensible, since they are so feeble. We should expect then, in this case, to have a less powerful red lasting a longer time, but with the gas flame a strikingly brilliant flush, lasting a very short time. Again, with a medium light the green and blue colours would be added rapidly, and the crimson flush would not be powerful enough to be conspicuous in that short time. I may say that in every point this agrees exactly with the appearances as they are really seen.

4. A Theory of Volcanic Eruptions. By Daniel Vaughan.

From researches which have much engaged my attention for nearly twenty years, I am convinced that silica performs a very important part, not only in the formation of the earth's crust, but also in leading to violent subterranean movements. The low specific gravity of silicic acid, and of the rocks in which it predominates, would (if much of the internal earth were fluid) give rise to certain results, which I traced in an essay published in 1856, and also in a paper which was brought before the British Association for the Advancement of Science in 1861. In the latter, I have given

reasons for believing that the invisible side of the earth's crust is very irregular in its structure; and that, as pressure promotes solidification, the great internal mountains must constantly increase in depression in consequence of the deposition on their peaks of solid matter of low density, and consisting either wholly or largely of silicic acid. I ascribed earthquakes to the occasional instability of such masses of new rock, as their size and buoyancy causes them to break loose from their fastening, and an ascending stony avalanche is driven against the weaker parts of the earth's crust.

But, on taking into consideration the great affinity of silicic acid for bases at a high temperature, volcanic phenomena may be traced to the collision of these silicious avalanches against such sedimentary rocks as contain carbonic and many other acids. Carbonate of lime, for instance, would not be decomposed by heat under the pressure it feels at great depths; but if a stratum of limestone were struck by a mass of incandescent quartz, or of highly silicified rocks, the resulting fragmentary mass would swell with the evolution of carbonic acid, and give rise to the various peculiarities observed in the eruptions and the upheaval of volcanic mountains.

5. On the Placentation of the Sloths. By Professor Turner.

After referring to the paucity of information on the placental characters of the sloths, and to the various inferences which had been drawn by anatomists from Carus's figure of the placenta of *Bradypus tridactylus*, some holding that it was cotyledonary and non-deciduate, others that it might have intermingled with it maternal deciduous substance, the author proceeded to describe his dissection of the perfectly fresh gravid uterus of a specimen of a two-toed sloth. This specimen, which was presented to him by Dr David Ridpath, only possessed six cervical vertebræ, and was referred to the *Cholœpus Hoffmanni* of Peters.

The author had succeeded in obtaining excellent injections both of the fœtal and maternal systems of blood-vessels. The placenta consisted of about thirty discoid lobes, aggregated together, and occupied about $\frac{3}{4}$ths of the surface of the ovum. These lobes could be peeled off the placental area of the uterus, and carried away with them a layer of deciduous serotina, the curling

arteries, utero-placental veins, and a very remarkable system of intra-placental maternal sinuses, continuous with the uterine vessels, freely anastomosing with each other within the substance of the lobes, and lying between and in contact with the fœtal villi. Definite walls, distinct from the walls of the fœtal villi, could be traced around the sinuses. Crowds of red blood corpuscles were situated within the sinuses, and it was observed that many of these seemed to be nucleated, an appearance which had been recognised a few years ago by Kühne, Rolleston, and Moseley, in the blood corpuscles of the Tardigrada. This sinus system possessed a special interest, because it presented a gradation between the capillary net-work of the uterine mucous membrane, occurring in the diffused placenta of the mare or the cetacean, and the freely anastomosing cavernous maternal blood spaces seen in the highly concentrated human placenta. The amnion lay in close contact with the inner surface of the chorion, as in the human fœtal membranes. The fœtus possessed a special envelope, like that figured and described by Welcker, as investing the fœtus of *B. tridactylus*, and named by him an Epitrichium. Numerous additional details respecting the structure of the placenta and membranes are contained in the memoir.

The conclusions drawn from the examination of this placenta were, that in the sloths the placenta is not cotyledonary and non-deciduate as in the Ruminants, but in the fullest sense of the word deciduate. If the inference drawn by Huxley from Sharpey's observations on the structure of the placenta of *Manis* be correct, then, if the placental system of classification is to be of any value, the non-deciduate scaly ant-eaters can no longer be grouped along with the deciduate sloths in the order Edentata, which order will have therefore to be subdivided. The author then compared the placentation of the sloth with that of the other deciduate mammals, and pointed out a series of very interesting affinities between its placenta and that in the Primates.

Monday, 2d June 1873.

SIR ROBERT CHRISTISON, BART., President, in the Chair.

The following Communications were read:—

1. On the Anatomy of a new species of Polyodon, the *Polyodon Gladius* of Martens, taken from the river Yang-tsze-kiang, 450 miles above Woosung. Part II., being its Nervous and Muscular Systems. By P. D. Handyside, M.D.

(*For a notice of Part I., see p.* 50).

The author showed to the Society a small entire specimen of the *P. gladius*, and next described, from a larger opened and dissected one, and from part of an adult fish, the spinal cord, the brain, the organs of the senses, and other parts of its nervous system. He illustrated his remarks by exhibiting four large drawings and nine smaller ones, including six microscopic views, explanatory of his description of the structure and disposition of the *spino-cerebral axis*, the encephalon as viewed from above and below, the ramifications of the encephalic nerves, and more particularly the structures subserving the senses of smell, sight, and hearing. A cartilaginous capsule forms *the olfactory* chamber, the mesial half of which is occupied by a fibrous disk composed of 29 septa which radiate from a prominent modiolus, and thus leave intermediate pituitary pouches, consisting of pigment cells and sarcole, invested with tapering, probably ciliary, epithelium. The choroid of *the eye* is connected with the exterior of the sclerotic by means of two large tubular processes that may be regarded, anatomically, as a modified form of the vaso-ganglion or choroid gland found hitherto in most osseous fishes only. The cysticule and utricule of *the auditory* apparatus are the only parts of the labyrinth that open into the cranial cavity,—differing thus from the generality of bony fishes and from sturgeons. A remarkable *sinus impar* is present, as in some osseous fishes; it is situated in the middle line of the skull, and connects the right and left vestibules through their upper walls.

Numerous cretaceous particles, of the nature of otolites, are studded over the interior of the walls of the cysticules and utricules; but none are found within the *sinus impar*, nor is the latter connected with either the air-bladder or with *atria* on the body of the atlas. Time did not permit of the author reading to the Society his paper in full.

The third part of Dr Handyside's paper will consist of an anatomical description of the viscera of organic life; and the fourth part, of the articular system and the endo-skeleton of the *Polyodon gladius*.

2. On the Placentation of the Seals. By Professor Turner.

After pointing out that the observations of Alessandrini, Rosenthal, Eschricht, and Barkow on the placentation of the seals had been limited to the determination of the form of the placenta, and to the more salient facts connected with the arrangement of the fœtal membranes, the author in this memoir proceeded to describe systematically the gravid uterus, the form and structure of the placenta, and the arrangement of the fœtal membranes of the grey seal, *Halichœrus gryphus*. He was indebted to Dr M'Bain and Captain Macdonald of the cruiser "Vigilant" for the opportunity of acquiring the gravid uterus of a recently killed specimen of this seal. The distribution of the utricular glands was described. The affinities between the placentation of the seals and the proper carnivora, more especially the common bitch, were pointed out. Differences in the degree of deciduation in the various forms of placentæ were considered, and it was shown that the seal, as regards its placental structure, occupied a position intermediate between the non-deciduate mare and cetacean, and the more highly deciduate forms of placenta.

3. Second Report by the Committee on Boulders appointed by the Society. (With a Plate.)

In April 1871, this Committee was appointed for two purposes —one to ascertain the districts in Scotland where boulders of interest were situated; the second, to point out such boulders as were deemed worthy of preservation, with a view to an appli-

cation to the proprietors of the land on which they were situated, to have them preserved.

The Committee, in fulfilment of the first of these objects, issued a number of schedules to the ministers and schoolmasters of Scotch parishes. The answers received enabled the Committee to present a First or interim Report to the Council of this Society; which Report was read at a meeting of the Society in April 1872.

The Committee have since continued their inquiries, and have obtained a considerable amount of additional information, the substance of which they propose to give in the following Second Report.

The additional information has been procured from three separate sources:—

1*st*, A considerable number of schedules, filled up by parochial ministers and schoolmasters, have been received by the Committee during the past year, several of which have been accompanied by sketches of the boulders.

2*d*, Special reports on particular boulders have been received from surveyors connected with the Ordnance and also the Geological Survey. These reports are particularly interesting.

3*d*, Your Convener, in a tour during last summer through some of the eastern and northern districts of Scotland, took an opportunity of inspecting some of the boulders mentioned in the schedules and reports received by the Committee, and ascertained many important facts.

The Committee, in order to record the information recently obtained, will follow the plan formerly adopted of specifying it for each county in alphabetical order, and in the briefest terms.

In now proceeding to explain the nature of the information received, the Committee desire to avoid as much as possible mixing up speculations with facts. But it is not easy to abstain from alluding to prevalent theories regarding the transport of boulders; nor would it be expedient to do so altogether, as it is desirable to show how far those theories seem to be supported or disproved by the facts ascertained.

I. Boulders.

1. From the statements appended to this and the First Report,

it will be seen that the boulders are divisible into two classes,—rounded and angular.

The boulders referred to are, of course, all "erratics," in the geological sense of the term, *i.e.*, they have been transported for considerable distances from a parent rock.

The rounded boulders are generally composed of rocks extremely tough and hard, and on this account were capable of undergoing great friction and rough usage without being broken up. Hence the round-shaped boulders most frequently consist of blue whin-stones, fine-grained granites, schists, limestones, and felspathic rocks.

The angular-shaped boulders, whilst embracing these rocks embrace also sandstones and conglomerates. Their angular shape of itself proves they could have undergone little or no rough usage by being rolled or pushed, for their angles and corners are in many cases sharp. Moreover, the rocks composing them are so loose and friable in texture, that they would have crumbled or been crushed to pieces had they undergone rolling or pushing.

The difference in shape between the two classes of boulders now referred to can be seen by looking at figures I., II., and V., as contrasted with figures IV., VI., and XIII. in Plate.

It may here, however, be proper to explain that some of the boulders composed of the friable rocks just mentioned, whilst angular and rough on one side, are sometimes rounded and smooth on the other,—a fact apparently indicating that after being carried to their present position without injury or mutilation, they had been subjected to friction or attrition on the particular side which is now smooth.

Some angular boulders are not cubical in shape, but are longer than they are broad; and in that case, the smoothed end, when there is one, is almost always narrower or more pointed than the rough end. It was a remark of Hugh Miller's, that on examining pebbles lying in the channel of a river, the great majority have their narrow ends pointing up stream. When once in that position, they retain the position longer than when lying broadside against the stream. This remark should not be lost sight of, in drawing inferences from the shapes and positions of boulders.

Examples of these angular boulders, with one end smoothed and pointed, will be noticed in Sketches VIII. and X.

It is not difficult to understand how boulders, originally cubical in shape, may undergo a change by the action of a stream of such a nature as to grind or smooth them. If fig. VIII represents a boulder cubical in shape when deposited, a stream coming against it in the direction of the arrow might break off or grind down the portion *a*, *b*, *c*, and leave the rest *a*, *c*, *f*, *e*, *d*, in the form of a boulder, smoothed and pointed at the *windward*, and rough at the *leeward* side.

That many of these boulders, after being brought to their present position, have been subjected to great attrition, is further proved by the markings on their surface. Scratches and sometimes deep ruts occur, not only on their upper surfaces, but also occasionally on their sides, as if indicating the passage over and along them, of stones harder than themselves, and pressed against them by some powerful agent or body. These scratches and ruts are most frequently in a direction coincident with the longer axis of the boulder, and show that the movement has been towards the boulder at its smooth and narrow end.

2. Another class of facts of some interest is connected with the positions of the boulders.

The rounded boulders, though frequently on the surface, are also, and perhaps more frequently, buried in mud, gravel, and sand-beds.

The angular boulders are occasionally found in these deposits; but they are much more frequently on the surface.

Angular boulders are very frequently on knolls or low hills, perhaps even more so than on lower levels. They are occasionally seen in clusters upon or round these knolls, as if the agent, whatever it was, which transported the boulders, had been obstructed in its further progress by the knolls, and had dropped them there.

As an example of this class of cases, reference may be made to the hill of Craigiebarns, about 1½ mile north of Dunkeld. This hill is about 1100 feet above the sea, and about 800 feet above the river Tay, which flows along its base. Four or five large boulders, mostly angular, lie on the top, or very near the top, of several rocky knolls which form the ridge of that hill. In like manner, there is a hill to the south of Dunkeld, also on the east bank of the Tay, with an angular boulder on the top of a rocky knoll.

Rounded boulders also occur on these rocky knolls, but not so frequently as angular boulders.

In connection with the fact of boulders being much clustered on and round rocky knolls, it may be noticed that boulders of large size, and especially angular boulders, are said to occur more frequently at high levels than at low levels.

On this point reference may be made to the report by one of the Ordnance surveyors from Boleskien in Inverness county, in which it is remarked that the hills in Stratherick reach to a height of 2900 feet, that the boulders are often perched on isolated hills, and that few boulders occur there below the level of 2250 feet.*

One of the largest angular boulders seen by your Convener (in Glen Lyon, Perthshire, and weighing above 100 tons), is at a height of 2500 feet above the sea.

It is proper, however, to add, that clusters of boulders do likewise occur at or beyond the mouths of valleys. In the Lochaber district, opposite to Loch Treig, in Spean Valley, there is a great accumulation of large boulders. So also in the valley of the river Nairn, to the east and below the mouth of Flichity Valley, there is a similar accumulation. In both of these cases the boulders lie on the top of debris, having all the appearance of moraines. These accumulations of boulders may be ascribed with great probability to the operation of glaciers. But that explanation cannot apply to clusters of boulders on or near the tops of hills.

3. Special notice deserves to be taken of the fact, that boulders of all sizes occur on islands, though in these islands no rock exists of the same nature as that composing the boulders.

In the First Report of the Committee several cases of that kind were reported from the Hebrides, as also from Orkney and Shetland.

In the statements appended to this Report, additional examples are reported.

Mr Campbell of Islay, in a document given in the Committee's last Report, says that many of the boulders on the western islands are "perched on hill tops," and have come from the northward in a direction "parallel to the run of the tides."

Appended to this Report, there is a similar opinion expressed, founded on the appearances in the small island of Foula (Shetland).

* See Inverness, page 157

The gentleman who expresses this opinion, and reports on the Foula boulders, affirms unhesitatingly, that these boulders came from the mainland of Shetland, separated by a deep sea of from 16 to 18 miles in breadth; and he even specifies the hill from which the boulders have come.

So also the report from the Lewis is distinct, that there are boulders on the east shores of the island which must have come a distance of at least 35 miles across the sea from the mainland of Sutherland.

The important bearing of these cases of island boulders on the nature of the transporting agent, is evident. It is difficult to see how glaciers could have been instrumental in carrying *them*.

4. The information recently obtained by the Committee throws additional light on the *direction* from which the boulders have come.

(1.) As already mentioned, some boulders appear to have come down valleys, brought apparently on glaciers. Two localities are mentioned, viz., in Lochaber and in Nairn Valley, where such explanation may be accepted.

There are also probably places in Perthshire, Forfarshire, and Aberdeenshire, where boulders lie in valleys, or at the mouths of valleys, which belong to this class of cases. The nature of the rock composing the boulders being found to be the same as the rocks *in situ* at the head or along the sides of the valleys, the birth-place of the boulders may be readily and correctly assumed to have been there.

(2.) But there are hundreds of localities with boulders, to which this explanation is inapplicable. Not only boulders situated on islands, but boulders perched high up on hill-sides and near mountain-tops, seem to require a different explanation.

On the mainland of Scotland, at least in its eastern half, from which the Committee have received the fullest information, the angular boulders, and also many of the rounded boulders, appear to have come over a wide extent of country in one and the same direction, viz., from the north-west, crossing valleys and ranges of hills.

This inference had been drawn, years ago, from such facts as the finding of granite and mica-schist boulders in the counties of

Fife, East Lothian, and Berwick, which must have come from the Scottish Highlands. But farther and more striking proofs of this great north-west movement are afforded by the large boulders in the counties of Elgin, Nairn, and Ross-shire, of which accounts are given in the Appendix to this Report. From these, it appears that conglomerate boulders, from 30 to 50 tons in weight, now lying on the hill-sides and the plains of Elgin and Nairn, must have been somehow transported from the conglomerate hills of Cromarty, across what is now the Moray Firth; and that granite boulders, very little smaller, and many of them angular, situated in the district between Tarbat Ness and Tain, at various levels up to 1200 feet above the sea, must have been transported from mountains far to the north-west.

The non-occurrence of conglomerate boulders in this last-mentioned district is also itself negative proof corroborative of the north-west movement;—there being no conglomerate hills to the north-west of the last-mentioned places.

Whilst the existence of this north-west movement is indicated by the birth-places of the boulders, other circumstances confirm the conclusion. Thus, most of the boulders now referred to have their smoothest and sharpest ends towards the north-west; the scratches and ruts on their surfaces and sides point in the same direction; and where there are striæ on the rocks of the district near these boulders, these striæ also, in nine cases out of ten, are parallel.

It also deserves notice, that when boulders are on hills, they evidently indicate a preference for the sides of those hills having a north-west aspect,—a fact which seems to indicate the existence and prevalence of some transporting agent which could be more frequently and effectually stopped by the sides of hills facing the north-west.

The facts reported from Elgin and Ross-shire bearing on this point are corroborated by the position of the boulders described as on the hill on the north side of the Linnhe Loch at Fort-William.

But whilst there is strong evidence, so far as the Committee have proceeded in their inquiries, to show, that in a great part of the mainland of Scotland a general movement has prevailed from the north-west, some facts indicate a separate movement from

the north-east. These are supplied partly from the Islands (Hebrides, Orkneys, and Shetland), and partly from the striations of rocks in different parts of Scotland. (See Reports from Lanark, Elgin, and Lochaber.) This north-east movement, however, does not appear to have been so general or so incisive as the north-west movement.

The Committee think it premature to draw conclusions with any confidence. They would only observe, that if two separate movements have taken place over the country, whereby rocks were striated, and boulders transported great distances across valleys, mountain ranges, and arms of the sea, it is most probable that these movements took place when the whole country was under the waters of a sea loaded with ice, and in which strong currents prevailed.

II. Beds of Clay, Gravel, and Sand.

1. Under this head, the most interesting fact brought out in the Reports lately received, is the occurrence at very high levels, of beds of boulder clay, gravel, and sand. They are to be seen in several parts of Scotland (chiefly the middle and north), at heights exceeding 2000 feet above the sea.

It can scarcely be doubted that the formation of these beds is due to large bodies of water. No marine organisms, it is true, have been found in these beds at or near this height; but it is difficult to account for them, except on the supposition that the whole country had been submerged to the depth of 2000 or 4000 feet. Currents, probably loaded with ice, have acted on the submerged mountains, and carried away from them an immense amount of debris, which has been deposited as sediment in the hollows, and forming the existing beds of sand, gravel, and clay.

When the land emerged, and for a long period after, there would be numberless lakes in the interior, among the mountains, formed of course by the rain which fell on their sides. In the course of time, the embankments of detrital matter which kept in the lakes would be cut through, and the lakes would sink in level. Most probably, when they so sank, beach lines would become visible on the sides of the mountains, like the famed parallel roads of Glen Roy.

Of these old beach lines there are many examples elsewhere than Glen Roy; and it is important to obtain reports of them, that they may be carefully examined.

Such terraces are visible in several parts of Flichity Valley, about 10 miles south of Inverness; and there also, the old detrital embankment still exists, which appears to have kept in the waters of the lake. It has been cut through by the river which now flows through Flichity Valley.

The whole of this valley deserves more particular examination, both with reference to these terraces, and with reference to the boulders, which lie in great heaps below its mouth.

In the valley of the Tay, between Pitlochry and Killiecrankie, there is a very instructive deposit of boulder clay and sand lying on the clay-slate rocks of Craig Ower hill, which forms here the east side of the valley. The deposit is well seen in two ravines, formed by mountain torrents, which have cut through the beds down to the rocks in one of the ravines, forming scaurs from 50 to 80 feet high. These scaurs show that, whilst the boulder clay presents only faint traces of stratification, if any, the beds of sand, which are in the heart of the boulder clay, are distinctly stratified. Following the course of the most northern of the two ravines up from the Pitlochry road (about 350 feet above sea, and about 150 above the bottom of the valley), he found the above deposits all the way up, to a height of about 1350 feet above the sea. He saw that this deposit was continued along the valley towards the north, and he was informed by the Rev. Mr Grant, of Tennandry, the minister of the parish, who takes some interest in these investigations, that similar deposits exist on the flanks of Ben-y-gloe, a mountain three or four miles to the north-east, within the limits of this valley, and at levels several hundred feet higher.

This locality was also visited by another member of the Committee, the Rev. Mr Brown, who was much struck by the beds of sand and clay before referred to. He states that he made a minute search for organisms in the clay, thinking that if the beds resulted from marine currents, some remains, either animal or vegetable, would exist, but he found none.

Whether these beds were formed by the sea or by fresh water, it

may be impossible at present to determine. But there seems strong reason to believe that the whole valley in this quarter was originally filled with detrital matter, which has since been carried away by the action of streams and rivers, except at a few places.

2. A question of considerable difficulty arises here in connection with the ancient glaciers which undoubtedly existed in Scotland.

Take, for example, Flichity Valley. If it was filled with a glacier which pressed on its sides, and carried down debris to form moraines, and the huge boulders which now lie on that debris, at what period did this glacier exist? Was it before or after the submergence of the country? If it was after the emergence, is it not likely that all the drift deposits of sand and gravel now on its sides would have been scoured out, and all traces of the terraces obliterated?

On the other hand, if the glacier existed before the submergence, is it likely that the moraines, in that valley, in Lochaber, and other parts of Scotland, would have retained so distinctly their prominent features? Would they not have been planed down by submarine currents?

It is, however, a circumstance in favour of the existence of glaciers before the submergence, that the striæ on rocks, which these glaciers are supposed to have produced, are often covered over by thick beds of sediment.

The Committee abstain from venturing farther upon theoretical ground. They allude to these questions only because the facts already ascertained, and more of which they will search for if they are re-appointed, seem calculated to throw upon these questions important gleams of light.

III. Work still before the Committee.

1. The Committee think that as so much information has been obtained from the east half of Scotland, it would be desirable to obtain similar information from the west half.

It is manifest from the Reports, that in the sea lochs of the west coast, there are boulders of large size, and in most interesting positions.

They are also particularly anxious to have as many particulars

as possible regarding boulders on islands, especially if the boulders are composed of rocks not existing in the islands.

IV. Preservation of Boulders.

The Committee have not yet taken any special action towards the attainment of this object.

Perhaps it may be premature to do so, till they have obtained all the information which they expect regarding the localities where the most interesting boulders are situated.

Some doubt also is felt what would be the most judicious course of procedure. In Switzerland, as the Committee observe from the printed Reports which Professor Favre kindly sends to them as they are issued, very many boulders have been purchased by or for natural history societies and museum managers; and in one of the Reports, a form of the deed or conveyance is given, transferring a right of property in any particular boulder.

From these Reports, it appears that the boulders sought to be preserved are—1*st*, Those which have a traditional name. 2*d*, Those which have a legend attached to them. 3*d*, Those which possess scientific value,—for some reason which geologists point out.

Perhaps the Committee, in their selection of boulders to be preserved in Scotland, could not do better than act on these principles. But whether, to secure preservation, they will endeavour to obtain a transference of the property of particular boulders in favour of any Society, or whether they will merely endeavour to obtain from the proprietor on whose lands they are situated, a promise to preserve them, the Committee have yet to decide.

The Committee cannot conclude their Report without repeating the wish, which they expressed last year, that some of the many tourists who are likely to be, during the ensuing summer and autumn, in remote parts of Scotland, where large boulders still exist, may visit the boulders, with a view to report upon them to the Committee. The Committee, whilst desirous of obtaining additional information from all parts of the country, may be allowed to add, that there are three classes of boulders, as regards position, information about which would be particularly acceptable :—

First, Boulders on an island, at a considerable distance from the mainland, when these boulders have evidently been transported to the island.

Second, Boulders at very high levels on the mainland, or on or near the tops of mountains, to which they have probably been transported from a distance.

Third, Boulders along the north-west coasts of Scotland, say 50 miles on each side of Cape Wrath, with the view of ascertaining whether these boulders have come from the mountains inland, or whether (as believed by some geologists) they have come from some region sea-ward.

The Committee, in drawing attention to the importance of obtaining information regarding the boulders situated on the north-west coast cf Scotland and on remote islands, know the difficulty of reaching these places by any of the means of conveyance accessible to the general public. There are, however, many gentlemen who have steam-yachts frequenting the western shores during the summer and autumn; and if they are disposed to assist this Committee in their researches, they might perhaps be induced to propose to a geological friend to accompany them on a cruise among the Hebrides, and give them an opportunity of visiting boulders not otherwise accessible. The Committee would be most grateful for any reports obtained in this manner, and would be very ready to acknowledge the service thereby rendered to geological research.

List of Boulders, Rocks, striated or smoothed, and Kaims, &c. reported to Royal Society Committee, arranged by Counties and Parishes.

ABERDEEN.

Kemnay.—Boulder, a fine grained blueish-grey granite called "The Souter's Stone," lying apparently in muddy sediment. Dimensions above ground 18 × 14 × 9 feet; but believed that as much below as above surface. Height above sea about 500 feet. Probably weighs 270 tons. This block on S.E. side of a hill running N. and S. for 500 yards, about quarter mile distant, top of which about 100 feet above boulder, and lies S.E.

of boulders described in Appendix to Committee's First Report. Country not suited for any glacier which could have brought "Souter's Stone," or any of the others. If "Souter's Stone" came from westward, it must have been floated and swung round by eddy into its present position. All the boulders in Kemnay and Chapel Garioch rounded and smooth on north and west ends, and rough at opposite ends. (Sketch of Souter's Stone, Plate No. II.)

Argyle.

Ardchattan.—Granite boulder, 14½ × 12½ × 5½ feet, partially rounded. One rut on top running whole length. Nearest rock of same kind is Ben Vreck, 3½ miles to eastward. Height above sea, 57 feet. Within 70 yards of boulder, a ridge of sand and gravel. Length, 1½ miles. Height of ridge varies from 50 to 100 feet. (Captain White, R.E.)

Ayr.

Ardrossan.—Near Hunterston on shore, boulder 5½ × 11 × 6 feet and 26½ feet round, apparently grey compact granite, about 12 miles from Arran, and opposite to Great Cumbrae Island, 1½ miles distant. (Robert Hunter, Hunterston.)

Berwickshire.

Berwick-on Tweed.—Castle Terrace. Boulder clay cut through for water pipes, and many boulders found, all more or less rounded, and composed of very hard rocks,—such as granites, gneiss, limestone, blue whinstone, greywacke, &c. The granites showed two varieties,—the common small-grained grey and red. (Observed by Convener.)

Burnmouth.—Near the railway station, among gravel, over greywacke rocks, a well-rounded lump of pinkish granite found by Convener.

This variety recognised by him as very similar to that used for a handsome chimney-piece in British Linen Company's Office, Edinburgh. Having ascertained that this chimney-piece supplied by Macdonald of Aberdeen, Convener sent to him a chip of boulder, that he might mention in what parts

of Scotland the rock was found *in situ*. Mr Macdonald replied,—"The light-coloured pinkish granite in the British Linen Company's Bank came from the hill of Correnie in Aberdeenshire, not far from Kemnay. It was cut from boulders. But similar rock is to be had quite near. Rock very much the same appears about Kincardine O'Neil, in Deeside, and also about Ballater and Braemar. A similar stone is found at Beaufort, county Mayo, and most likely in other parts of Scotland and Ireland.

"Your boulder is very much akin to all these rocks; perhaps a little closer in the grain, but substantially the same.

"Many of the ocean-worn beach stones all along the coast to the south of this, are of the same granite as your boulder, or very much like it.

"I cannot lay my hands on a specimen sent to us, some years ago, from the Island of Uist; but if recollection bears me out truly, granite very much of the same character is to be got there."

Coldstream.—At the Hirsel (Earl of Home). About 120 feet above sea, boulder of white chert about 4 feet square, but very rough and irregular in shape. Found in a bed of gravel in making new avenue from Hirsel Policy to Coldstream. No rock of this nature known on north side of Tweed. It occurs at two or three places along the south of the Tweed, viz., at Carham and Nottylees; places bearing W. by S. from boulder, distant about 3 miles, with Tweed valley intervening.

As rock composing boulder friable, and its shape very angular and ragged, it could not have been rolled or pushed to its present site, nor could have been thrown down from any great height. Probability is, that when detached from parent rock, it fell upon ice, which floated it across valley.

Dunse.—On farm of Cockburn, a boulder of mica-schist well rounded. Is from 2 to 3 feet in length and breadth. An erratic from the Highlands of Scotland, and must have travelled at least 100 miles across many valleys and ranges of hills.—(First noticed by Mr Stevenson of Dunse.)

Foulden.—Several small boulders of coarse sienite (lying on old red sandstone), composed of red felspar, black hornblende, and

small flakes of mica—nearest hill where similar rock, Cockburn Law—8 miles to N.W. Largest boulder, 5 × 3½ × 3 feet. Longer axis, N.W. Sharpest end points that way. (Convener.)

Greenlaw.—Marchmont (Sir Hugh H. Campbell). About 930 feet above sea, a blue whinstone boulder 9½ × 5 × 4½ feet, with faint striæ on top, parallel with longer axis. Original position of boulder slightly changed before being seen. This boulder must have come from westward. Rocks *in situ* Old Red. District not favourable to glacier theory. (Sketch by Lady Hume Campbell.)

Hutton.—Boulder of whinstone about 12½ tons found in clay of brickwork. Longer axis W.N.W. Sharpest end towards that direction. Probably from Hardens Hill, west of Dunse, 10 miles distant W.N.W. Striæ parallel with longer axis on one side. This boulder now in Paxton Policy grounds.

Buteshire.

Big Cumbrae.—Rev. Mr Lytteil, Kilmarnock, pointed out to Convener many boulders of mica-schist on many parts of island. Rocks of island are Old Red. Largest boulder seen is near north end of island, at Balloch Martin, 12 × 6 × 3 feet. But as much more probably below ground. Longer axis N.N.E. Lies in a trough or valley running N.N.E. May have been floated through this valley from northward.

Little Cumbrae.—Rev. Mr Lytteil conducted Convener to highest part of island, north of Old Tower, 400 feet above sea. Rocks (claystone trap) here smoothed by some agent passing over from N. by W. Found several boulders of conglomerate and Old Red; none of mica-schist. Largest about 5 feet square, and rests on trap rock, by so small a basis that it may once have rocked. Known by name of "Bell Stane." Mr Lytteil supposes name derived from "Beltane" fires lighted here in Pagan times. Close to this stone, another smaller conglomerate boulder, with cup on it, apparently artificial, 4 inches diameter and ½ inch deep. Height above sea, 190 feet. Situated about 2 miles N.W. of old castle on east shore.

No old red or conglomerate rocks in island. Nearest are along shore at Rothesay, about 20 miles across sea to N.W.

Visited "Split Boulder," first mentioned by Smith of Jordanhill. A claystone trap, similar to rock of island. Lies at sea-level on rocks smoothed and striated, forming east side of a trough or valley running N.E. by N. Striæ run same way.

Dumbarton.

Luss.—Mica-schist boulder on west bank of Loch Lomond, 26 × 18 × 7 feet; about 250 feet above sea-level. Situated on a brook entering Fruin Water, west of Callendoun Farm House. Longer axis E. and W., with sharp end to west and thick end to east. Rocks adjoining, old red sandstone. Nearest mica-schist hills to north and west, about 5 miles off. Reporter remarks, that if boulder came from W. or N.W., it must have crossed hills from 1000 to 2000 feet high. But it may have come from north, down valley now occupied by Loch Lomond, on ice which floated it so far south, and then carried it west up Glen Fruin.—(R. L. Jack, F.G.S., Alexandria.)

Elgin.

Dyke.—Near west end of approach to Darnaway Castle, several granite and gneiss boulders from 2 to 3 tons each.

In same parish, near west lodge to Darnaway Castle, a kaim, quarter mile long, running N. and S. 12 feet above general surface. (Captain White, R.E.)

Elgin.—Boulder called "Carlin's Stone," on Bogton farm,—a coarse conglomerate, about 230 feet above sea. Imbedded pebbles, chiefly flesh-coloured quartzite.

About half a mile to N.W. a smaller boulder, called "Young Carlin's Stone."

Conglomerate rock occurs in hills to south, distant 5 or 6 miles; but of a variety different from that of boulders. Same variety of conglomerate as the boulders exists beyond Inverness to W., and in Ross-shire to N.W.

From size and shape of these conglomerate boulders, evident they must have been *carried* or *rafted*.

On other hand, in this district hundreds of round and smooth boulders of granite, gneiss, mica slate clay, slate, &c., whose shape and smoothness indicate that they have been *pushed*

or *rolled* over the surface. These chiefly imbedded in gravel, clay, and sand.

To westward of these boulders a valley or depression runs in an E. and W. direction; Halldon or Pluscardine Hill being on south, and Carden Hill on north side. This valley opens out to westward.

Pluscardine Hill on its north slope dipping towards valley, covered with boulders which apparently deposited on it from some agent that came from N.W., and which obstructed by the hill.

Carden Hill has a flat ridge running about E. and W. This ridge consists mostly of a bare and hard gritty sandstone rock. It has been evidently ground down and smoothed by hard and heavy bodies passing over it. Striæ observed in numerous places on Carden Hill, viz., W. by N., N.W., N.W., N.W. by W., N.W. by W., N.W. by W. The average direction was N.W. by W., and from the formation of striæ, agent which produced them, evidently came from north-westward.

Numerous boulders along ridge of hill of granite (chiefly grey, one of red), gneiss, &c. The red granite boulder $4\frac{1}{2} \times 2\frac{1}{2} \times 1\frac{1}{2}$ feet. Its longer axis N.W. by W., and its sharpest end was towards that point.

Most of these boulders rounded and smooth, as if great friction and pressure had operated on them.

Some were lying along ridge on its northern slope, as if arrested in their further progress. Numbers also along ridge on south slope, as if pushed over hill, and put into positions where beyond reach of pushing agent.

At one place, sandstone rocks of ridge broken up, as evidenced by great fragments lying along southern slope, where beyond the reach of agent which broke and pushed them. These sandstone blocks lie at levels about 40 to 60 feet below level of ridge.

This flat ridge of Carden Hill extends for about a mile, and is about 400 feet above sea.

From ridge of this hill, the Carlin Stone boulder above-mentioned seen, bearing S.S.E. about two miles distant. It is not probable that it came over Carden Hill. More probable,

that it was floated through long valley between Pluscardine and Carden, in which case its course would be E.S.E., in conformity with prevailing movement in this district. Bottom of this valley from 130 to 140 feet above sea.

At one place on top of Carden ridge, N.W. striæ crossed by striæ from N. by E.; at another place, W. by N. striæ crossed by striæ from N.W. The N. by E. striæ seemed the older.

These variations in direction of striæ more reconcilable with idea of drift-ice than with glaciers.

Moreover, in this district no possibility of any local glacier from N.W.

From Carden Hill, Cromarty Firth bears about N.W., distant about 20 miles across sea. If a glacier brought these boulders from Ross-shire, it must have crossed Moray Firth, and risen over Carden Hill, and passed across valley on south of it obliquely.

If land submerged beneath an Arctic sea, and a N.W. current prevailed, possible to understand facts observable in this district.

Quarrywood Hill, about 200 feet above sea, and composed of sandstone striated on top. On N.W. slope four or five large conglomerate boulders about 140 feet above sea. Apparently from Ross-shire, and obstructed in further progress by this hill. (Convener much indebted to Mr Martin, Elgin, for pointing out facts above stated.)

Forres.—Conglomerate boulder, $9\frac{1}{2} \times 8 \times 8$ feet, about 44 tons, called "Doupping Stone," from legend of ceremony in admitting Forres burgesses; situated on Upper Caliper farm, about 580 feet above sea. Rock composing boulder evidently same as Carlin's Stone, near Elgin, being characterised by liver-coloured quartz nodules. This boulder situated on hill-side fronting Cromarty, which bears N.W. by N. across Moray Firth about 10 miles.

Informed by tenant of farm, that another boulder of same kind higher up hill, but so buried in earth that only upper part visible.

Forres to Nairn.—Extensive beds of sand and gravel, mostly stratified, shown in railway cuttings. Pebbles and boulders

in these beds always rounded and smooth, seldom angular. Angular boulders apparently never imbedded entirely, almost always on surface.

Lossiemouth.—On old sea margin, 20 feet above present sea level, conglomerate boulder same as Carlin's Stone. About 1½ miles west of Caussie Lighthouse, a large boulder of silicated sandstone on a hill sloping to N.W. with N.W. striæ on it.

Inverugie limestone quarry, strata dip rapidly to north. On surface of rock, striæ running E. and W. This deviation from normal direction perhaps caused by dip of stratum.

In boulder clay over limestone rocks here, boulders of oolite found, which probably came from Ross or Sutherlandshire across Moray Firth.

Portion of an Oolite boulder seen by Convener, which found near Duffus school-house, about 125 feet above sea.

"Witch Stone," about quarter mile west of Duffus school, 250 feet above sea, viz., a large conglomerate boulder, exactly similar to Carlin's Stone, containing nodules of granite, gneiss, and purple-coloured quartz. Its longer axis N.W., and sharpest end towards that quarter. Hill on which lies, slopes that way. Lies on bed of sand.

On Clarkeley Hill, 1½ miles eastward of Burgh-head, hard sandstone rocks striated N.W. On same hill, several boulders of granite (both red and grey) and gneiss, lying on hill sloping to N.W. One of them, 4 × 3 × 2 feet, has its longer axis in same direction. They could have come only from N.W., and therefore across sea. (Rev. Dr Gordon, Birnie, pointed these out to Convener.)

HEBRIDES.

Iona.—Convener found on east side of island at the shore, small well-rounded boulder of conglomerate. Heard that similar boulders to be seen on west shore of Iona in St Columb's Bay. Conglomerate rocks, said to be *in situ*, at Inch Kenneth Island, forming cliffs 50 feet high, about 10 miles to N.E. The rocks in Iona are clay slate.

N.E. of Cathedral, on shore, hundreds of granite boulders (chiefly red variety). Several exceed 20 tons in weight.

Farther north, in a cultivated field, about 50 feet above sea,

red granite boulder, weighing about 150 tons, called the "Geadh," or "Goose," 24 × 18 × 6 feet. Longer axis, S.E. (See Plate, Sketch No. IX.)

About ½ mile farther north, on east shore, another large red granite boulder, about 12 feet square. East end rests on clay-slate rocks of island. West end rests on a smaller granite boulder. Rocks below boulder decayed out, so that possible to creep under boulder. Groove on bottom of boulder running N.E. as if pushed over rocks from that direction. This boulder very rounded at angles, apparently from friction.

Boulder of red granite on side of highest hill in island, called "Dun Ii." Cubical in shape, and very angular, 22 × 16 × 16 feet. Boulder lies against steep slope of hill facing N.N.W., at height of 230 feet above sea.

Convener found red granite boulders of smaller size, at height of about 400 feet, the highest point of island.

Mr Allan M'Donald, schoolmaster, says that these granite boulders seem to be of same variety as that in Ross of Mull; but he thinks granite does not occur there so high as 300 feet. Ross of Mull bears from this spot S.S.E. The "Dun Ii" hill lies between Ross of Mull and this boulder.

When asked by Convener if any red granite in Islands of Tiree or Ulva, to north of Iona? Mr M'Donald said there was none.

Rocks on Iona more smoothed at the highest levels than at lower levels.

Smooth faces of rocks front N. by E. The rough faces all front south.

At south end of Iona a number of granite boulders (mostly red, but a few of grey variety) lying on the high ground, from 200 to 250 feet above sea. One of these standing up on end, leaning against a rock on S.W. side of boulder, showing that boulder came from N.E., and was obstructed by rock in its farther progress to S.W. (See Plate, Sketch No. XI.)

Most of boulders in south end of Iona lie with longer axis, N.E. and E.N.E.

Some of the boulders in this district in such positions, that they could not have come into them, except by floating ice, brought from northward, and by eddying currents.

In Ross of Mull granite of both red and grey varieties extensively quarried.

Heard of a large boulder on west side of island, in two fragments, which said to suggest idea of having been broken by falling from height.

Lismore Island.—Convener found several boulders of granite, both red and grey, which supposed to come from the Kingairloch hills to north. Almost all the large boulders broken up.

Staffa.—Convener found several small boulders of red granite on surface. No rocks of granite *in situ* here.

Stornoway.—Boulder 15 × 7 × ? of old Cambrian rock, very hard, and close in texture. Boulder now blasted. Rested on gneissic rock, and differed from any rock in the Lewis. Height above sea, about 50 feet.

The whole hill at back of Nether Pyble strewn with small round stones of similar Cambrian rock.

In parish of Ness, from Lighthouse at the Butt, thousands of small worn boulders of Cambrian rock scattered over surface, even on highest points.

No Cambrian rock *in situ* nearer than mainland. The rock *in situ* gneiss. (Henry Caunter, Stornoway.)

Inverness.

Boleskien, Abertarff, and Dores.—1. Granite boulders of red and grey varieties, in great numbers, over district of Stratherrick. Well rounded. Largest, near farm-house of Dell, 20 × 10 × 7 feet above ground, and apparently as much below ground. Longer axis, N. and S. Another near Fall of Foyers, 12 × 6 × 6 feet above ground. Granite (grey) occurs *in situ.*

2. On hills from which rivers Foyers and Ness rise, a great many boulders of granite and schist. The granite boulders well rounded; the schist boulders angular. Several perched on tops of isolated hills.

Highest hills in district about 2900 feet above sea. The boulders extend down to a level of about 2250 feet. Few below this level except in beds of streams. (Captain White, R.E.)

Culloden Muir.—Duke of Cumberland's "stone," a conglomerate

boulder with six sides, girth altogether not quite 60 feet, and height 6 feet. Longer axis W.N.W. Height above sea about 470 feet; a few faint traces of striæ running W. by N. Nodules seemed similar to, but not quite the same as, those of Elgin and Nairn boulders.

Croy (Village).—About ½ mile to S.W., and 320 feet above sea. A mica schist boulder 17 × 9 × 9 feet. Lies on hill sloping to N.W. (Convener.)

A kaim begins here, which said to run eastward through counties of Nairn and Elgin for 30 miles.

A conglomerate boulder called "Tom Riach" (Plate, Sketch I.), of following dimensions:—West side, 18 feet; north side, 21 feet; east side, 24 feet; south side, 21 feet; height, 20 feet. It stands in the middle of a plain or flat valley through which River Nairn flows. Rocks *in situ* are gneiss, and boulder apparently rests on this rock. A small portion of its under surface, resting on the rock, visible. It looks smooth, as if it had been pushed over the subjacent rocks; and there seemed grooves or scorings which coincided with axis of valley, which here E. and W. This boulder must have come from distance, and been carried by ice, of some kind. In higher parts of valley in which this boulder occurs, conglomerate rocks *in situ* exist. Not probable that this boulder could come from N.W., as in that case it would be carried over Culloden Muir, which 300 feet higher than boulder. Very probable that brought by glacier from westward. Ingredients of this conglomerate apparently not same as those in Elgin and Nairnshire.

On high plateau, 4 miles south of Inverness, at height of about 774 feet above sea, another conglomerate boulder, with a thin stratum of old red sandstone on top. Girth about 51 feet. Height, 9 feet. Longer axis N. and S. Kaim of gravel and sand to N. of boulder, about 900 feet above sea, running E. and W. being direction parallel with valley of Nairn. (These boulders shown to Convener by Mr Jolly, Inverness.)

Dallanossie (Parish).—Moy Hall estate, Dallry farm, Mr Fraser, tenant. Boulder, 30 × 18 × 9 feet, apparently a bastard granite; though rocks of adjoining district are also granitic, the boulder much darker in colour. Nearest rock *in situ* S.W. by S. about

a mile distant. Boulder split into two unequal parts. Its Gaelic name is "Clach Schuilt"—meaning "Cloven Stone." Height above sea, 2090 feet. (Captain White, R.E.)

Duntelchak Hill, west of Inverness.—Top about 900 feet above sea. Rocks composing it, a coarse conglomerate. On N.W. side of hill, rocks ground down and smoothed;—on S.E. side of hill rocks rough and steep.

A granite boulder lying on N.W. slope of this hill, about 30 feet below top. Length 7 feet, width 4 feet. Longer axis N.W., and sharp end towards that quarter. (Convener.)

Flichity Valley.—Beds of sand and gravel seen on hills to south, about 1500 feet above sea. Not near enough to be examined. At east or lower end of valley, top of a rocky hill striated, in direction parallel with axis of valley, viz., E.N.E.

At Farr, in Nairn Valley, a continuation of Flichity Valley, near the Free Church, a most remarkable assemblage of boulders. Some rounded, but most of them angular. Many are about 7 feet square. No conglomerate boulders here;—all gneiss or mica schist. They mostly rest on gravelly detritus, which may have been moraines. Others (and these are round shaped) rest on a smoothed rocky surface of gneiss beautifully glaciated, and sloping down towards west—*i.e.*, looking up valley. (See Plate, Sketch No. XIII.) This glaciated rock—smooth towards west, and dipping at angle of 30°—is on its east side rough and vertical. Very manifestly these rounded blocks, glaciated rocks, and gravelly debris, indicated glacier action. Two valleys meet here, one (Flichity) bearing due west, the other (Duntelchak) bearing N.W. Both valleys deserve exploration, with reference to remains above specified.

At lower end of Flichity Valley (about 3 miles west of these boulders), a great embankment of gravel and sand, through which River Nairn has cut passage about 200 feet deep. Before this passage cut, a lake must have filled Flichity Valley, dammed back by the gravel accumulation. That such a lake existed, proved by terraces on hill sides of valley. *Query*, If a glacier filled this valley, and brought blocks and moraines to Farr Church, when did this occur? Any gravelly embankment, such as now exists at east end of Flichity Valley, would

have been swept away by a glacier. Glacier must, therefore, have existed, and disappeared, before embankment formed.

The only solution of problem seems to be, that after glacier had filled valley, carrying down blocks and debris to Farr, land sank under sea, destroying glacier, but not disturbing position of boulders, or carrying away much of moraines.

In this Flichity Valley an isolated hill, about 1620 feet above sea. Near the top of hill, rocks (gneiss) on W. and N.W. sides present an appearance of having been rubbed and ground down; on its W.N.W., S.E., and E. sides, boulders of gneiss attract attention, not only from size, but from very precarious positions. Boulders evidently erratic, for though gneiss, they are different kind of gneiss from that forming hill, and, being rounded, they have undergone considerable friction before reaching present position. The hill remarkably precipitous where boulders situated, insomuch that if they had fallen from any height, they would have rolled down. To prevent this, boulders must have been brought close to side of the hill where now lie, and let gently or gradually down upon hill-side.

A sketch is given of one of these boulders, to show how near it is to a precipitous portion of the hill. (See Plate, Sketch IV.)

These boulders about 500 feet above bottom of the valley.

In descending from this hill top, along the north side four several horizontal terraces passed, separated from one another by about 100 feet less or more, having appearance of old beach lines. On these terraces the boulders more numerous than elsewhere. (Mr Jolly of Inverness, was guide to Convener).

On N.W. of Craig Phædrick Hill, Inverness, the hard conglomerate rocks bared, rounded, and smoothed, and sloping towards N.W., at about 500 feet above sea; on south side of hill, same rocks rough and vertical.

Transported boulders of gneiss, &c., on N.W. side of hill—none elsewhere.

Many of these boulders have sides sloping down to N.W.

On several parts of hill, especially east side, rocks (old conglomerate, coarse and compact) broken up into huge cubical masses, similar to Tomriach boulder (see page 158)—many much larger.

Above Clachnaharry, grooves on rocks, E. and W.—a direction parallel with Beauly Valley; might have been made by glacier descending valley, or by drift ice, if valley submerged.

A remarkable boulder here, 30 paces in girth, and about 15 feet high, and roughly estimated to weigh 100 tons;—probably that called by Anderson "The Watchman's Stone." Name very suitable, as it rests on a projecting part of coast, and extensive view from it. Situated on what appears a terrace of drift, about 73 feet above sea.

A very extensive sea-terrace, about 40 feet above sea, girds coast of Beauly Firth, and seems to be repeated at Lentran and Clunes Railway stations, towards Tain.

Fort-William.—Ascended hill on north side of Linnhe Loch. Along both sides of loch several terraces visible, running for some distance one above another,—viz., 20 feet, 110 or 120 feet, and 494 feet above sea. This hill, called Treshlik, covered by small pebbles, indicative of aqueous action.

This hill about 1566 feet above sea. It forms a ridge about ¼ mile long, running W.S.W. and E.N.E. Rocks on north and west sides smoothed, as if by friction of some agent passing over them from W.N.W. No such appearances on any other side of hill.

But these smoothings confined to a line along hill, not reaching lower than about 60 feet from top, nor reaching higher than about 30 feet from top.

Large boulder of coarse granite on N.W. angle of hill about 1494 feet above sea. It is about 16 paces round, and about 8 feet high. Boulder rests on the edge of the stratified rocks of hill, viz., clay slate. (See Plate, Sketch No. VI.)

The boulder in composition resembles boulder on Cluny M'Pherson's lands. (See next page.)

This boulder on very precarious site. The hill here exceedingly steep. Boulder could not have been brought from any eastern point; for in that case, it would have rolled down hill.

It probably did not come from a point due west, because Blythe Hill bears due west, about 2 miles distant, and forms a large mass about 2500 feet above sea, which would intercept

any agent moving towards Treshlik Hill. There are only two points from which boulder probably came, viz., from mouth of Linnhe Loch bearing from Treshlik Hill S.W. by W., or from Loch Eil bearing from Treshlik Hill N.W.

The preference must be given to the N.W. quarter, because of the numbers of other coarse granite boulders all along the north slope of hill, and of the smoothed rocks being on same side.

Highland Railway from *Forres to Kingussie* cuts through immense deposits of clay, gravel, and sand, up to a height of about 800 feet above sea. Some of these deposits are stratified. They are full of rounded blocks of all sizes.

At *Dava* station, on east side of railway, about 900 feet above sea, rocks facing N.W. show large extent of surface glaciated.

At summit level, viz., about 1080 feet above sea (north of Kingussie), stratified gravelly drift abounds.

At *Kingussie* (about 730 feet above sea), two sets of terraces visible on the sides of valley, one about 50 feet higher than other, indicating existence of a lake at some former period, and which had been drained by barrier confining it having been cut through by Spey. Ruthven Castle stands on an isolated mass of drift, which probably formed island in this ancient lake at summit level of railway, viz., between Dalwhinnie and Dalnaspidel, about 1430 feet above sea; beds of sandy gravelly detritus abundant, apparently remains of aqueous sediment; where cut through by burns, they form scaurs or cliffs 50 to 60 feet high. Mr Robertson, factor, Old Blair, states, in letter to Convener, that near summit level of railway at Drumnachdier and Dalnaspidel, there were extensive deposits cut through of "pure sand," "so fine and soft" that it could not be used for building. He adds, that at a spot a little higher, viz., 1480 feet above sea, there was found (from surface)—1*st*, A peat bed, 2 or 3 feet thick, containing *fir* roots; 2*d*, A layer of clayey gravel about 2 feet thick; 3*d*, A peat bed with decayed branches of *birch* and *hazel*, and no fir; 4*th*, Tilly gravel.

On Cluny M'Pherson's lands (about 6 miles west of Kingussie) two large boulders of a very coarse grained granite on south side of Spey. One boulder is 11 × 9 × 6 feet. Plates

of mica in boulder about 1 inch square. Felspar, green in colour. Longer axis E. and W. This boulder lies on hill side sloping down to west. Height above sea about 1035 feet. The other boulder, about double size of previous one, about ¾ mile N.E. from it, and at height of about 1080 feet above sea; also on hill side sloping down to west. There are other boulders on this hill of smaller size.

Rocks of district are a variety of coarse clay slate.

The only hill in this district is Craig Dhu, situated to north about 4 miles, consisting of clay slate, and about 2500 feet above sea.

Nearest granite rocks situated to westward. In that direction a valley, down which these boulders might have come. The physical features, however, not favourable to glacier theory, from absence of any range of hills to southward.

At *Laggan Free Church*, a well-rounded granite boulder, lying on a glaciated and striated rock of clay slate, sloping down to west, facing upper part of valley. Boulder 9 × 6 × 6 feet; longer axis E. and W., which corresponds also with striæ on rock, and with general direction of valley at this place.

Kingussie.—Boulder called "The Big Ordan Stone," said to be whinstone, situated on hill 5 miles S.W. of Kingussie, and 2 miles from Newtonmore Railway Station, on Belville estate and farm of Etteridge. Shape angular. Longer axis, S.S.W. Has a deep hollow on top facing S.W. Greatest length (viz., on S.E. side), 13 feet 10 inches. Breadth at top, 8 feet 4 inches. Height, 8 feet 10 inches. No similar rock in district. Height above sea from 950 to 1000 feet. One legend is that Fingal used the stone for a putting-stone, throwing it from Craig Dhu, on opposite side of river Spey; another, that when Fingal wished to drink out of the Spey, he put one foot on Craig Roy (a low shoulder of Craig Dhu) and the other on Ordan Hill, but finding Ordan too low, he threw the boulder from Craig Roy that he might put his foot on it. (John Robertson, Old Blair.)

Inverie.—On road toward Arrar, about 2 miles to north, and at

height of about 360 feet above sea, clay slate rocks smoothed and striated in a direction from N.W. by N.

Fell in with two boulders lying near each other on side of hill, sloping down to W.N.W., where sea situated, about one mile distant. In one case, boulder lying on clay slate rocks; in the other case, boulder so sunk that base not visible. First boulder 8 × 6½ × 4 feet. Longer axis N.W. Second boulder 9 × 5 × 4 feet. (Shown to Convener by James Baird of Cambusdoon.)

Both boulders apparently came from N.W., and intercepted in further progress by hill.

Fell in with another boulder, which broken into two fragments. Configuration of district indicates that it must also have come from N.W. Smaller fragment lies from rest of boulder at a distance of 4 or 5 feet and to S.E. A study of fragments creates impression that boulder has been broken, not by action of frost, but by falling from a height, which caused concussion.

On shore to west of Inverie House, several boulders of coarse granite, similar to Fort-William and Cluny M'Pherson granite. These Inverie boulders supposed to have come from Dunedin and Cairnmore Hills, about 10 miles to eastward, and at head of valleys opening to west coast. Opinion expressed to Convener that these boulders not so likely to have come down the Dhulochan valley as the Loch Nevis valley.

At Invergussern (about 8 miles north of Inverie), the valley has been, at its mouth near the sea, crossed by an immense embankment of gravel and sand, about 30 or 40 feet deep, lying over rocks.

The river has cut through this embankment, and also a portion of the rocks covered by it.

This embankment probably terminal moraine of a glacier or a submarine deposit, more probably the latter, as sandy, and in some places stratified. Its ridge is about 140 feet above sea. At one time it has served purpose of a dam to keep in lake, the successive levels of which are seen on both sides of valley.

At summit-level between Inverie and Gussern, viz., from 400

to 500 feet above sea, a flat or terrace visible, with a number of boulders on it.

KIRKCUDBRIGHT.

Borgue.—Boulder of red sienitic granite. Oblong in shape. Longer axis N.W. and S.E. Rests on a low hill of partially decomposed trap. Longest sloping side fronts N.W. The S.E. end vertical and rough. Girth at 3 feet above base is 23 feet. A line over and across boulder measures 16 feet. Rocks *in situ* at and near boulder are partly trap, partly greywacke.

No granite nearer than about 10 miles, forming a range of hills extending from Dalbeattie, east of boulder, to Creetown, west of boulder. (See Plate, Sketch No. XII.)

Formerly many similar boulders in parish, all now broken up. (Earl of Selkirk and Rev. Geo. Cook.)

LANARK.

Glasgow.—Near Possil, sandstone rocks covered by boulder clay. Two sets of striæ on rocks under boulder clay—viz., from N.W. and from N.E.; oldest from N.W., and caused by a more powerful agent, judging by length and depth of striæ. Boulders in clay, recognised by Mr John Young (Hunterian Museum) as from Kilpatrick hills to N.W., and Campsie hills to N.E.

At Brickwork, Garscube Road, sandstone rocks also striated from N.W. more deeply than at Possil. No striæ from N.E. Perhaps striating agent here intercepted by a hill to N.W., quarter of a mile distant, about 100 feet high. At this place, in boulder clay, numerous boulders of old conglomerate, grey granite, schists, &c., from Bonaw and Kilpatrick hills to N.W. (Convener.)

NAIRN.

Auldearn.—1. Conglomerate boulder called Grass Stone, 15 × 9 × 4 feet, rounded. Longest axis, N.W. Height above sea, 200 feet.

2. Grey granite boulder 6 × 5 × 4 feet, a few yards S.E. of No. 1, round and quite smooth.

3. Red granite boulder, about 1½ miles south of Nos. 1 and 2.

Lowest axis N.W. Size 12 × 8½ × 8 feet; striated in various directions. Well rounded. 350 feet above sea.

The rocks of district old red. Nearest place, where rocks same as boulders occur, is in Ross-shire to N.W.

Thousands of smaller boulders, similar to the above, scattered over district, used for buildings. (Captain White, R.E.)

Kaim of gravel and sand, with steep sides. Average direction east and west, winding in usual serpentine way. Is here continuous for ¾ mile. Average height above adjoining ground 30 feet. Full of well-rounded and smooth pebbles and boulders from rocks of district.

Auldearn (Parish).—*Brightmoney*, Lathan Estate, south of Dalmore Free Church. Five conglomerate boulders all on ground, sloping towards N.W., about 200 feet above sea, and 1 mile distant. Partly buried in sandy drift.

Their longer axis N.W. They slope towards that quarter, and have a smooth surface; whilst S.E. ends rough and steep. If these blocks were originally, when brought to spot, cubical, as when detached from parent rocks, they would have this shape. If any strong current loaded with ice were to come from N.W., their angles on N.W. end might be broken off, so as give shapes they now have. (See Plate, Sketch No. VII.)

Cawdor.—Hill of Urchany, composed of granite rocks. Nevertheless, blocks of old red sandstone scattered over surface in such quantities, that used for building houses and dykes. These must have come from north, as sandstone rocks only in that quarter, about 2 miles distant, and at a lower level.

The following four conglomerate boulders seen:—

1. "Clach na Gillean," or "Young Man's Stone." Height 10 feet, and girth 54 feet. Height above sea, 687 feet. Some of its corners angular, on crest or summit level of Urquhany hill. (See Plate, Sketch No. V.)

2. "Clach na Cailleach," or "Old Wife's Stone," on same hill, but on side which slopes to west by north. Height 15 feet; girth, 54 feet. Height above sea, 581 feet.

3. "Clach an Oglach," or "Boy's Stone." Lies at east end of a kaim. Height, 9 feet; girth, 69 feet; above sea, 312

feet. (A gneiss boulder near it about one-fourth the size. Numerous smaller do.)

4. Oblong conglomerate boulder lying on a bank facing W.N.W. Longer axis W.N.W., 50 feet × 24 × 12 feet. (Shown to Convener by Mr Stables of Cawdor Castle.)

Orkney.

Mainland.—Mr Miller of Bin Scarth says, that a valley runs E. and W. across the mainland of Orkney, forming in its course the bed of the Lochs of Stennis and Stanay. There is no large boulder in this district, but on north exposure of the hills, there are small stones strewed over the surface, quite different from rocks *in situ*. The former are a white bastard freestone; the latter, old red sandstone or flag pavement.

There is evidence through all this valley, of it having been channel of a tidal strait. There are in it hummocks of sand, mud, and water-worn gravel. Below these, reporter found heaps of small sprigs, brushwood, and hazel-nuts, preserved in moss, similar to the submarine mosses and forests under the bays of Otterswick, Deersound, &c.

The comparatively recent elevation from under the sea of all this district, is evident. Traces also exist of dry land with forests and other produce not now suiting climate.

Reporter does not know of any large boulder in the Orkneys, except on Sanday Island.

Sanday.—Dr Smith, secretary to the Edinburgh Royal Physical Society, sends to the Committee the following extracts from a MSS. paper by the late Dr Patrick Neill, on the Shetland Islands, dated 26th January 1806:—

"'Moorstone of Sanda,' Island of Sanday, flattest and lowest of the Orkneys. Greater part only a few feet above sea. Near a place called *Saville,* and not far from Burness Parish Church, stands a large isolated mass of primary rock—an aggregate of quartz, whitish felspar, and black mica. These disposed in layers, so that when seen in the mass, they constitute a block of gneiss. I did not accurately make measurements, but roughly estimated weight at 12 or 13 tons.

"Rocks of Sanda are wholly secondary strata,—sandstones, sandstone flag, breccia, and limestone.

"The only primary rocks in Orkney are in the largest islands (Mainland or Pomona), close by sea-port of Stromness, above 30 miles distant from Sanda. Hill at back of Stromness seems granite, with outer coating of gneiss. The gneiss, which is similar in quality to the Sanda Moorstone, is traversed by dykes or veins of granite.

"About a mile to N.E. of Stromness secondary strata begin. From thence to Sanda only sandstone and limestone visible.

"From Stromness to Burness Church is at least 34 miles in a direct line.

"On supposition that this gneiss tumbler in Sanda formed part of Stromness hill, it must have passed over 15 miles of what is land, and 19 miles of what is sea, at present.

"The firths of Westra and Eda, between Stromness and Sanda, are of immense depth,* through which the waters of the Atlantic now rush with indescribable force towards east or German Ocean, at the ebbing of the tides."

Dr Neill adds that he cannot imagine how this boulder transported from Stromness to Sanda, except by "what Saussure has termed a debacle," "the rush of vast torrents," which, besides transporting the boulder, might "have also scooped out those hollows which are now the firths of Westra and Eda."

Stromness bears from Saville about W.S.W., and a straight line between the two places crosses not only several firths, but several islands. If the boulder came from Stromness, as supposed by Dr Neill, its transportation by *land* ice is inconceivable.

Statistical Account states that granite rock, passing into gneiss, runs through Stromness parish, forming a tract about a mile wide, and six miles long (vol. xv. p. 46); and that all the rest of Orkney Islands are sandstones of different kinds. It is added, that "rolled blocks of granite are found in these islands far from their original position" (page 210).

Whilst it is very probable that this Sanda boulder came, as

* Admiralty charts show depths of water in these firths to be from 10 to 20 fathoms.

Dr Neill supposed, from Stromness, it is right to keep in view that granite and gneiss rocks abound to the N.N.W. in the Shetland Islands. Transportation by *land* ice from these remote islands seems also inconceivable.

Mr Miller, schoolmaster of Cross and Burness, reporting the above boulder in Sanda to Committee, says that it is 22 feet in girth, and is round in shape.

Stromness (Parish).—Two granite boulders lying on old red sandstone near manse. A range of granite hills six miles long situated to eastward. One of boulders is a mile, the other a quarter of a mile distant from these hills. One boulder 50, the other 100 feet above sea. Each boulder 3 or 4 feet in length, breadth, and height. (Reporter, Rev. Ch. Clouston.)

PERTH.

Blairgowrie.—Two miles west of town, on road to Essendy Bridge, a Druidical circle of 5 large mica-schist boulders, about 5 feet long, and 6 or 7 feet in girth.

Another boulder, 7 × 5 × 3 feet, lies on summit of steep acclivity on Woodhead Farm.—(W. S. Soutar.)

Granite boulder, 4 × 3½ × 3 feet, on side of Ericht, quarter mile N. of Blairgowrie, excavated in making mill lead. No rock of same kind nearer than 30 miles in Braemar range of hills to N.W. Height above sea, 200 feet. Numerous granite blocks found in excavating for foundations of houses in Blairgowrie.

Callander.—Gneiss boulder called "Samson's Putting Stone" on top of Bochastle Hill, two miles west of Callander, 14 × 9 × 9 feet. Longer axis N.N.E. Lies on coarse old conglomerate, viz., same bed or stratum which crosses Scotland from Dumbarton to Stonehaven. Boulder, judging by nature of rock composing it, must have come from north-westward. It occupies precarious position, being close to edge of a precipitous face of hill about 330 above valley, fronting W.S.W. towards Loch Katrine. It may have been lodged either by a glacier which descended from Loch Katrine, or by floating ice, when land submerged. About 50 feet below the above boulder, and on a very steep part of hill, another boulder.

$6 \times 4 \times 4$ feet, very angular, of gneiss, evidently also brought from westward. Several quartz boulders on hill, which also must have come from westward. (Convener.)—(See Plate, Sketch No. IV.)

Clunie.—Gneiss boulder $8 \times 5 \times 4$ feet, with longer axis S.W. Gneiss boulder $10 \times 6 \times 5$ feet, with longer axis N.W. Both boulders on tops of knolls, and must have come from Grampians 5 or 6 miles to N.W. down a valley. First boulder called "The Grey Stone." Height about 320 feet above sea.—(Robert M'Leish, schoolmaster.)

Dunkeld.—Craigiebarns Hill, to N.E. of town, visited; made it about 1000 feet above river Tay at Dunkeld Bridge, and about 1250 feet above sea.

Several boulders of mica-schists at and near top of hill, but chiefly on sides facing N.W. Rocks *in situ* also mica-schist, but not the same variety as boulders.

These boulders mostly angular and sharp in edges; only one or two rounded; among these one of a hard brown sandstone.

Greater number of boulders perched on top or sides of knolls than in hollows. Agent which transported them had been of such a nature as to be interrupted in its progress by knolls, and made to discharge its cargo of boulders on them. (One of these boulders shown in Plate, Sketch No. X.)

On this hill, rocks smoothed and striated in numerous places. These markings, when examined minutely, show a movement over the rocks, to produce them, from N.N.W. The longer axis of boulders, generally N.N.W., which is towards head of valley. But whether a glacier occupied valley, or floating ice, not clear.

On Craigiebarns, gravel found at the highest points.

On descending hill towards the river, observed on rocks the following directions of striæ, at the height specified:—

At 972 feet above river,	striæ,	direction of,		N. by W.
„ 700	„	„	„	N½E.
„ 648	„	„	„	N. by E.
„ 288	„	„	„	N. by E.

The axis of the valley at this place N.E.; therefore agent which produced striæ, seems to have been of such a nature

as to fill valley; this agent in its upper part (where it overtopped sides of the valley) moved obliquely across valley, but in lower part (near the bottom of valley) it followed course of valley.

Hill on east side of River Tay, 2 miles S.E. of Dunkeld about 1200 feet above sea. Gravel and sand abound all over it, to top.

On a knoll of clay slate, saw boulder of gneiss about 650 feet above sea. It was on side of knoll facing N.N.W. Longer axis of boulder also N.N.W., and its sharpest end in that direction.

Saw another boulder of gneiss 6 × 3 × 2½ feet, lying on well-smoothed slate rocks. Longer axis N.W. Height above sea 1000 feet. Rocks evidently smoothed from N.W.

Some of rocks on this hill show smoothings from two separate directions; one from north (as if down Tay Valley), the other from west (as if down Bran Valley). The rocks of clay slate are exceedingly hard, so that their smoothing indicates tremendous friction.

A very extensive flat stretches south from Dunkeld about 260 feet above sea. Robert Chambers notices it, and says it is 280 feet above sea. A terrace at about the same height, visible on hill, skirting Tay on east side, half a mile S.E. of Dunkeld. Probably the sea formed both.

Fowlis.—Abercairney estate. Granite boulder weighing about 30 tons; about 500 feet above sea. Situated on north side of a valley running E. and W. Rocks *in situ* old red. This boulder, and some smaller near it, must have come from northwestward. May have come either by a glacier or by drift ice. Granite hills about 20 miles to N. and N.W. (Rev. Mr Hardy, and Convener.)

Glen Lyon above *Invervar.*—Gneiss boulder (called "Clach-na Salainn," from people who brought trees out of Black Wood of Rannoch resting them on the boulder), composed of six or seven large fragments. The whole mass about 30 yards round and about 3 yards high. May weigh about 120 tons. Rests apparently on coarse gritty sand. Must have been brought to present site by ice, and from northward. Height above sea

about 2500 feet. On south side of summit level, between Glen Lyon and Rannoch, a cliff called the Scaur, half a mile to N.W. of boulder. Rocks *in situ* clay slate. If boulder came, as seems probable, from W.N.W., transporting agent must have passed on one side or other of Scaur. Configuration of hills here not favourable for glacier. Boulder within about one mile of summit level, which probably only 200 to 300 feet higher than it. Boulder may have been broken up by action of frost, or by having fallen from transporting agent. First theory not probable, as interior surfaces of fragments appear as weathered as any of the exterior sides of boulder.

About 500 feet below boulder, on banks of river Var, thick beds of boulder clay, sand, and gravel, full of rounded boulders, indicating aqueous deposits.

Hill on east side of Var, facing west, much covered by boulders, as if brought from westward by some agent, whose progress intercepted by hill. One of these boulders, known by name of "Clach na Tarbh," or Stone of the Bull.

Killiecrankie.—On east side of Killiecrankie Glen, on Fascally Estate, two ravines, parallel to one another, show very high cliffs of detrital matter full of large boulders. In the southmost of the two ravines, the scaurs are about 100 feet high.

These scaurs in the higher parts of the ravines show sections of stratified sand and fine gravel to a large extent. Traced these up to a height of about 1570 feet above sea (by aneroid). Was told by Rev. Mr Grant, of Tennandry, that at or near the hill of Ben y Gloe, there are beds of sand and gravel at a still higher level.

Some of the boulders in the most northern of these two ravines, which had fallen out of the drift deposits, were of large size; one, on being measured, showed 12 × 6 × 5 feet.

The following kinds noted:—Granite, grey, fine grained; granite, red, very coarse grained; gneiss, quartz, porphyry, limestone, primitive.

There is a large angular limestone boulder at the Pass of Killiecrankie, about ½ mile north of Tennandry mass, sticking in boulder clay about 856 feet above sea. These limestone

boulders supposed to have come from Ben y Gloe, or some other mountains to the north.

Killin.—Ascended hill west of, made it 1350 feet above Loch Tay, and therefore about 1650 feet above sea. Sides of this hill, at least that facing eastward, covered with sandy detritus; but could not discover whether stratified or not. This detritus here reaches to foot of some steep rocky crags, at height of about 1000 feet above loch. But on adjoining hills descried through telescope, sandy deposits at least 500 feet higher.

At height of about 1090 feet above loch, rocks of hill exhibited effects of friction by action of some body pressing against them from a direction W. by S., viz., down the valley. Rocks facing east were uniformly rough.

If it was a glacier which effected this smoothing, the drift deposits on hill sides must belong to a period subsequent in date, as glacier would have scoured them all away.

On north side of Loch Tay, an extensive flat about 400 feet above loch, with appearance of a similar flat on opposite side.

Schehallion ascended.—Rock composing it, a very hard sandstone. The hill forms a long ridge running E. and W., the highest part of which at west end, viz., about 3560 feet above sea. The side of hill which seemed smoothest, faces N.W. by W.; but no striæ, or even any very clear proofs of a grinding action, seen. Gravel, indicative of aqueous deposition, seen up to a height of about 3000 feet.

Various small blocks of a fine grained grey granite scattered over surface up to a height of about 3000 feet. A similar rock said to be *in situ* at Loch Sunart to N.W.

On south side of Schehallion, at a height of about 2500 feet, rocks apparently ground down and smoothed, but not above this level.

In cliffs of the Burn courses on the south side of Schehallion, boulder clay noticed, up to a height of 1500 feet above the sea.

All the strath between Dunkeld and Pitlochry seems to have been a lake. Bottom of this lake indicated by a flat, through which Rivers Garry and Tummel have cut, to present channels. This flat is about 50 feet above these river courses

at Ballinluig. Some miles farther south, this flat is from 80 to 100 feet above river. The barrier must have been to north of Dunkeld.

At the lower end of *Glen Tummell*, about ½ mile west of Bonskied House, there is a large amount of debris, exceedingly like a terminal moraine, with boulders lying upon it. But there was no opportunity to examine the locality.

Struan Railway Station.—Two boulders of gneiss on southern slope of hill, on left bank of the river Garry, east of station. Rock in channel of Garry also gneiss, but not exactly same. One boulder 12 × 6 × 7 feet. Longer axis N.E. and S.W. The other boulder 7 × 8 × 5 feet. (John Robertson, Old Blair.)

Ross.

Edderton.—Three large boulders of grey granite inspected by Convener, on hill south of manse, about 1000 feet above sea. These boulders on side of a hill sloping to N.N.W.

No. 1, about 910 feet above sea, and about 80 feet below a col or lowest part of the mountain range, called the "Stranger's Stone." If land submerged 2000 feet, a current probably existed, which, if from northward, and bearing ice, might carry boulders, and when ice touched hill-side would discharge them. Hill-side very steep at this place, so steep as to make it difficult to understand how boulder deposited without rolling down.

No. 2 boulder. About 710 feet above sea. Is situated on a sort of flat or terrace. Its longer axis (about 16 feet) points N. by E. General slope of hill here about N.E.

No. 3 boulder. Translation of its Gaelic name is "Big Lair of Fox." Height above sea about 752 feet. Situated on a well-marked flat or terrace, which bounded on south by a steepish cliff. Longer axis N. and S. General slope of hill here is to N.

The above three boulders clearly transported. Composed of granite—a grey variety. Rocks of hill on which rest, old red sandstone.

Where have they come from? The surmise that they came from "Cairn na Cunneig" seems improbable, if, as alleged, it consists chiefly of red granite.

The Rev. Mr M'Ewen, of Edderton, suggested the hills at or near Rogart, due N. or N. by E. from this spot about 10 or 12 miles, as the rocks there of grey granite.

These boulders not favourable to glacier theory. Their elevated positions, and the absence of any hills to the west or north nearer than 5 or 6 miles, are circumstances which render that theory almost impossible.

Rosskeen.—Ardross. Numerous large boulders, their longer axis nearly E. and W.

No. 1. March stone between Newmore and Ardross, about 50 feet in girth, and 8 feet above ground.

No. 2. At Achnacloich, at road-side, granite boulder 40 feet in girth.

No. 3. About half a mile above Ardross Castle, by way-side in a dyke, about 100 feet in girth, and 9 feet above ground.

No. 4. In a field opposite No. 1, of similar shape and size.

District between *Tain* and *Tarbet Ness.*—Shows on surface neither boulders, nor gravel, nor sand, but traces of mud, and occasionally of boulder clay, visible. At Fort-George, boulder clay reported 100 feet deep and more. Mr Stables, of Cawdor Castle, bored into it for water to that depth, and did not get through it.

If land was submerged 2000 feet, district about Fort-George, Moray Firth, Dingwall, Cromarty, &c., would be deeper than adjoining districts, and would be filled with muddy sediment, whilst shallower districts would be covered with gravelly and sandy sediment. The valley now occupied by Loch Ness and Caledonian Canal then a strait or kyle, through which tidal currents would pass; and if icebergs and drift ice came from westward, boulders and debris would be deposited on what are now the low lands of Moray, Banff, Elgin, and Ross, with the intervening Firths.

At Tarbet Ness, Balnabruach boulder visited, in company with Rev. George Campbell; a coarse reddish granite 33 feet in girth, and about 7 feet high. Longer axis E. and W. This boulder and another, not quite so large, near it, at sea level. Supposed to have come from "Carn na Cunneig" hill, which visible from boulder, bearing W.N.W. about 30 miles,

with an area of sea between Tarbet Ness and coast near Tain, 10 or 12 miles distant.

An old sea margin visible here, about 11 feet above high-water mark, with several large boulders on it. When materials of old sea-cliff washed away by sea, these heavy boulders remained. Boulders on such terraces, when numerous, are thus indicative of aqueous erosion.

None of the boulders at Tarbet Ness are conglomerates. If transporting agent came from any points between W. and N.N.W. in Ross-shire, granite boulders both red and grey could have come to Tarbet. The conglomerate rocks are to the southward of the above line—viz., on Beauly River, higher parts of Strath Conon, Black Isle, &c.; and hence boulders taken from them, and moving S.E., would not cross Tarbet Ness, but would be carried towards Nairn, Elgin, and Banff-shires, where actually found.

One of boulders (near Fearn parish school) has attached to it a Gaelic legend, the translation of which as follows:—

> "Grey stone of the clay hollow
> Makes three sommersaults
> When it hears the cock crow."

This boulder slopes downwards on its north side. It is also towards north, that land most depressed—viz., towards the sea.

Boulder of red granite 2 miles north of *Tain*, on road to Edderton, called after Sir Walter Scott,—about 70 feet above sea level,—supposed to have come from Cairn na Cunneig.

SHETLAND.

Bressay.—A boulder of coarse white sandstone, 10 × 7 × 4½ feet, wholly unlike any other in parish. The rocks *in situ* old red sandstone, and in N.W. of island there is conglomerate or pudding stone. There was another larger boulder now split up.

A great many smaller boulders, viz., from 8 to 14 cwt. each, of the same sort of rock, viz., a white coarse conglomerate sandstone. These boulders are north of Lerwick, from one to two miles. (Reporter's name not attached to schedule.)

Rev. Dr Gordon of Birnie (an experienced observer) visited Shetland in September 1872, near Northmaven (the extreme

north of mainland). When there, he heard of large boulders half-way between Hillswick and Ollabery.

He has procured for Committee, pencil sketches of three boulders, situated in extreme north of the mainland between St Magnus' Bay and Yell Sound. They are syenitic, same as singular "stacks" in that district called the "Drengs" (needles). One near Eela-water, 16 × 12 × 6 feet; another, called "Crupna" (bent?), 11 × 8 × 8 feet; another, called "Bonhus," situated between the two others, is 8 × 10 × 10·8 feet.

In two places, about 20 miles asunder, he met with striæ on rocks;—one a mile north of the Fishing Huts of Stennis, on N.W. shore of St Magnus' Bay, on coarse conglomerate rock; the other at centre and bottom of a valley, about half a mile wide, and bounded by hills 200 or 300 feet high, near Maris Grind, in front of farm house of Islebury, on quartzose gneiss. At both places striæ were E. and W. (true). At Islebury, valley runs N. and S., so that the agent which striated rocks there, crossed valley at right angles.

Foula.—Five boulders from 3 to 5 cwt. each, and two boulders about 2 tons each.

The five are at Hametown in south end of island, and lie on the north side of what was a strait, when land submerged, but now a valley between the Noup Hill and Hill of Liorafield. "Of these five, two are granite from Culswick, and three gneiss from (I would say) the Delting Hills. The compass bearings of these places from the boulders are (I would say) N.E. These five boulders are as smooth as if taken off a beach a short time ago."

The two boulders of 2 tons each are in middle of island, and of irregular shape.

From middle of island to south end, and as high up as 700 feet, granite and gneiss drift; but had not time to examine the north end of the island.

"The drift must have come from either Culswick 16 miles, Norshaven 30 miles, or Delting 30 miles, borne along by tides similar to what we have now, and which set in the direction of Foula from the mainland.

"I shall send by and by a lengthened report, on drift in

Walls and Sandsting." (Rev. James Russell, Parliamentary Schoolmaster, Happyhousel, Walls.)

Statistical Account of Foula states that "Foula is composed of old red sandstone, with subordinate deposits of granite, gneiss, and mica slate. (Vol. xv. p. 20.)

Lunna.—Stones of Stoffus. Mr Irvine, schoolmaster, called on Professor Nicol, Aberdeen, and showed to him specimen broken from stones. It is ordinary grey gneiss, quite like common rock of the islands. He alluded to doubt whether "stones" transported. Professor Nicol inferred from Mr Irvine's account they had been transported. There is no higher ground near them, and they form a landmark from the sea. They are from 20 to 22 feet high, and 90 feet round. Height above sea from 100 to 120 feet.

Professor Nicol adds:—"When in Shetland, I saw almost no indications of glacier action, except near the Grind of the Navir in the extreme west, where the rocks are distinctly striated and polished."

Explanation of Lithographic Sketches of Boulders.

I. "*Tom Riach.*" (See page 158.)

II. "*Souter's Stone.*" (See page 149.)

III. "*Samson's Putting Stone.*" (See page 168.) This boulder is near top of hill, as shown on Sketch. The other and smaller boulder below it, on hill side, is also shown on Sketch. The shape of each is indicated on a larger scale in the Sketch.

IV. "*Flitchity Valley.*" Boulders near to top of hill, as shown on Sketch. The shape of one, and its precarious position, shown to right of Sketch on a larger scale. (See page 160.)

V. "*Clach na Cailleach*" Boulder, or "Old Wife's Stone." (See page 166.)

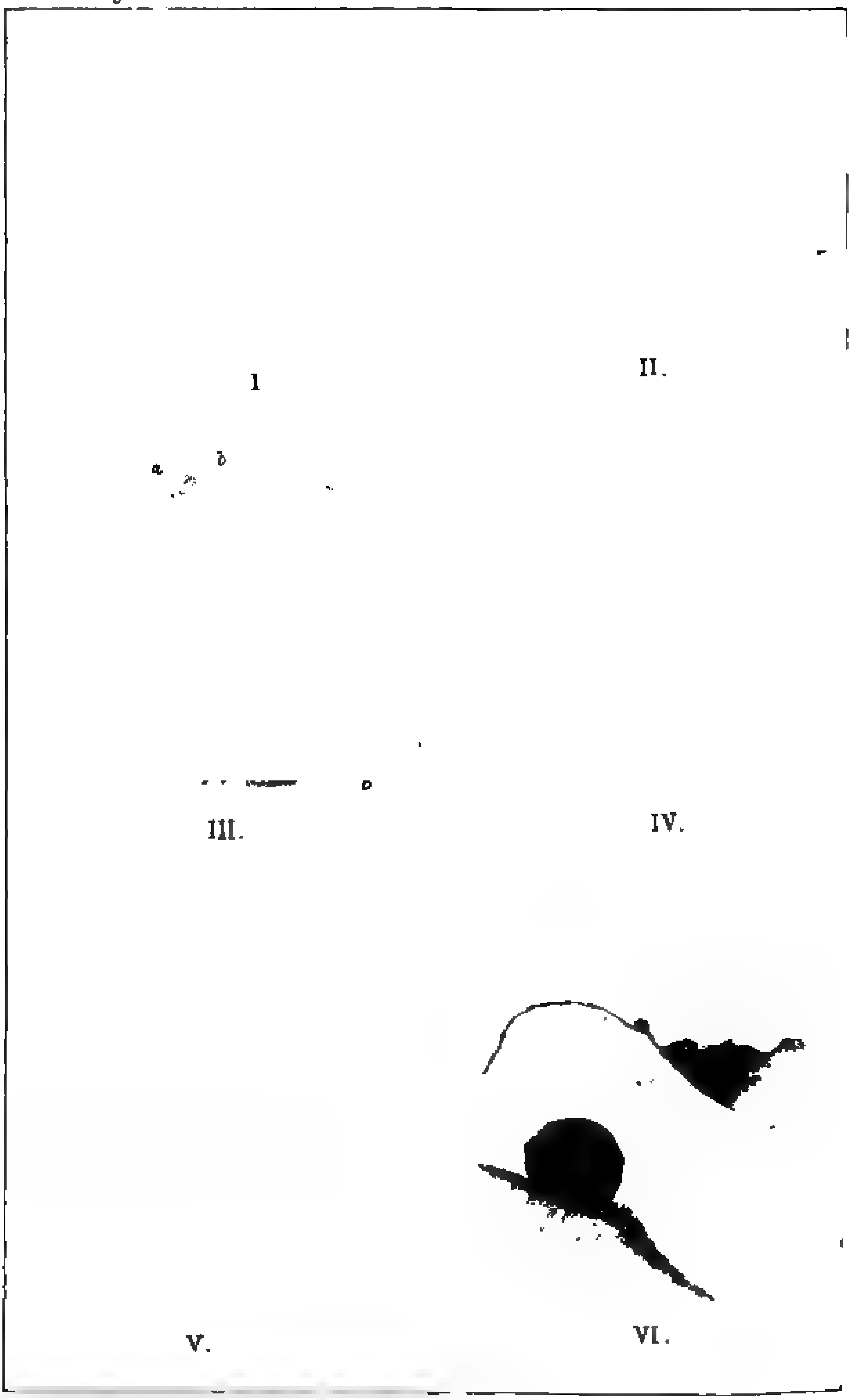
VI. Boulder on *Treshlik Hill.* (See page 161.) Sketch shows position of boulder near top of hill. There is also an enlarged view, to show shape of boulder, and its precarious site.

VII. Boulder on *Dun Ii, Iona.* (See page 156.)

VIII. *Auldearn.* This Sketch in the dark shaded part shows general shape of the conglomerate boulders mentioned on page 166. The faintly shaded part is intended to show what the original shape of boulder may have been. (See page 140.)

IX. "*Geadh*" or "*Goose*" Boulder, Iona. (Page 155.)

X. Boulder of mica slate, on top of a rocky knoll, at Craigie Barns, North of Dunkeld. Length, 7 feet; width, 5½ feet; depth, 4 feet. The smooth and sharp end points N. by W. The smooth surface


I
II.
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VII

IX

XIII.

McFarlane & Erskine, Lith. Edin.

of rock on which boulder rests slopes down also N. by W. This rock also mica slate, but of a variety different from boulder. Height of boulder above river Tay 855 feet. (See page 169.)

XI. Boulder near S.E. end of Iona, standing upright. (See page 156.)

XII. Borgue boulder. (See page 165.)

XIII. Two rounded boulders lying on glaciated rock. (See page 159.)

4. On the Physiological Action of Light. No. III. By James Dewar, Esq., and John G. M'Kendrick, M.D.

Since the date of our last communication, we have continued our investigations, with the following results:—

1. The light from a beam of uncondensed moonlight, though of weak intensity, and almost entirely free from heat rays, is still sufficient to alter the electro-motive power of the nerve and retina.

2. We have examined the phenomenon in the eyes of the following animals:—(1.) The common newt (*Triton aquaticus*). (2.) The gold-fish (*Cyprinus auratus*). (3.) The rockling (*Motella vulgaris*). (4.) The stickleback (*Gasterosteus trachurus*). (5.) The common edible crab (*Cancer pagurus*). (6.) The swimming crab (*Portunus puber*). (7.) The spider crab (*Hyas coarctatus*). (8.) The hermit crab (*Pagurus Bernhardus*), and (9.) The lobster (*Homarus vulgaris*).

The general results with the eyes of these various animals were similar to those we have previously described. The eye of the gold-fish and rockling, both sluggish fishes, were found to resemble each other, inasmuch as the variations in the electro-motive force were slow, and in this respect they presented a marked contrast to those of the active and alert stickleback, the eye of which was very sensitive to light.

The experiments on the eyes of crustacea are of importance, because they show that the action of light on the compound eye is the same as on the simple eye,—namely, that it alters the amount of the electro-motive force of the sensitive surface. The eye of the lobster was found to give a deflection of about 600 galvano-metrical degrees (the scale being placed at a distance of about 26 inches). Light produced a variation in this deflection of about 60 degrees—that is, about 10 per cent., the largest amount of variation we have yet observed in any eye. It was also demonstrated that the effect of light diminished in intensity by distance was exactly what was observed in the case of the simple eye. For example, at the dis-

tance of one foot, a variation to the extent of about 100 degrees was observed. At a distance of 10 feet, with $\frac{1}{100}$th part of the amount of light, the effect was not 1 degree, but 20 degrees, or $\frac{1}{5}$th of the total amount observed at 1 foot.

3. The action of light on the electro-motive force of the living eye in cats and birds (pigeon and owl) has been observed. In our earlier experiments we found great difficulty in observing sensitiveness to light in the eyes of mammals and birds when these were removed with the utmost despatch from the orbit of the animal immediately after death. This was evidently owing to the fact that the sensibility of the nervous system in these animals disappears quickly after the withdrawal of healthy blood. It therefore became necessary to perform the experiment on the living animal. This was done by first putting the cat or bird under the influence of chloroform, then fixing it by a proper apparatus, so that the head was perfectly immovable, and lastly removing the outer wall of the orbit with as little disturbance to the ciliary vessels as possible. The optic nerve was now cut, the transverse section directed upwards, and the clay points of the electrodes were now adjusted, one to the transverse section of the nerve, and the other to the cornea. With these arrangements we at once found a strong current extremely sensitive to light.

4. The effect was traced into the optic lobes of a living pigeon under chloroform. The following were the results of this observation: (*a.*) when one pole was applied to the left optic lobe, and the other to the cornea of the right eye, a deflection was obtained, which was sensitive to light; (*b.*) when the pole was removed from the right eye, and applied to the cornea of the left, a smaller deflection was obtained, also sensitive to light; and (*c.*) when light was allowed to impinge on both eyes, while the one pole was in contact with either eye, and the other with the left optic lobe, the result was nearly double that produced by the impact of light on one eye alone, either right or left. These effects may be explained by the decussation of the optic nerves in the optic commissure.

5. The eye of a snake * was examined, and in its action resembled that of the frog.

* Kindly sent us by Mr Bartlett of the Zoological Gardens, Regent's Park. We have also to acknowledge the kindness of Mr Lloyd, manager of the Crys-

6. We are therefore now in a position to state that the law of the variation in the electro-motive force of the retina and optic nerve holds good in the following groups of the animal kingdom: mammalia, aves, reptilia, amphibia, pisces, and crustacea.

7. Many experiments have been made which prove that the psycho-physical law of Fechner, alluded to in previous communications, is not dependent only on perception in the brain, but in part on the structure of the eye itself. The effects which occur on, during, and after the action of light on the retina, also take place after the eye has been removed from all connection with the brain. Thus the law of Fechner is not, as has been hitherto supposed, a function of the brain alone, but is really a function of the terminal organ, the retina.

8. We have also employed a new method of registering galvanometrical variations, which may be of service in many physical and physiological researches. This consists in placing at the proper distance from the galvanometer, instead of the ordinary graduated scale, the surface of a cylinder covered with paper, and moving on a horizontal axis by clock-work. The spot of light reflected from the galvanometer mirror is rendered more precise by having the shade of the galvanometer lamp blackened over the entire surface, with the exception of a spot about three millimeters in breadth, in the centre of which a line or cross is made of soot. The image of this line or cross is, of course, reflected by the mirror upon the cylinder. When the cylinder is set in motion by the clockwork, the spot of light may be accurately followed by the hand of the observer, after a little practice, with a fine brush moistened with ink. The cylinder we employed performed a complete revolution in 80 seconds. This time was divided into 4 equal parts, each representing 20 seconds, by 4 lines drawn transversely at equal intervals across the paper on the cylinder. The first space, between lines 1 and 2, represented 20 seconds, in which the eye was in the dark, and in which the electro-motive force is represented by a straight line; the second space, between lines 2 and 3, represented 20 seconds, during which the effect of the impact of light took place, and in which the variation of the electro-motive force is indicated either by

tal Palace Aquarium, who supplied us with three specimens of *Eledone* (a cuttle-fish, to represent mollusca), but none arrived alive.

a curve to the right or to the left; the third space, between lines 3 and 4, represented 20 seconds of continued action of light, during which the electro-motive force gradually rises; and, lastly, the fourth space, between lines 4 and 1 (the point of starting) represents 20 seconds, during which the electro-motive force at first rises on the withdrawal of light and afterwards sinks rapidly.

5. On the Thermo-electric Properties of Pure Nickel. By Professor Tait.

By the kindness of M. de Boisbaudran I have been enabled to experiment upon a specimen of nickel, very nearly pure. Its thermo-electric relations are exceedingly interesting, and are easily observed by employing palladium as the second metal in the circuit. The nickel line in the thermo-electric diagram presents nearly the same appearance as that of iron, but its peculiarities occur at much lower temperatures.

Speaking generally, at low temperatures it is nearly parallel to the palladium line, but below it; the specific heat of electricity being negative. The specific heat changes sign about 230° C., and thereafter the nickel line intersects the palladium. Shortly after this intersection (at about 340° C.) the specific heat again becomes negative, and of nearly its first amount; so that the lines are again parallel, but nickel is now above palladium. These curious facts are probably connected with the magnetic properties of iron and nickel, possibly also with the chemical distinction of ferricum and ferrosum. But exact determinations (which I hope soon to make) are required before such speculations can be successfully carried out.

6. Notice of the Ravages of the *Limnoria terebrans* on Greenheart Timber. By David Stevenson, Civil Engineer.

In 1862 I communicated to the Society a notice of the ravages of the *Limnoria terebrans* on timber employed in engineering structures exposed to the action of the sea. In that communication I stated that African, English, and American oaks, mahogany, teak, beech, ash, elm, and the different varieties of pine, were found sooner or later to become a prey to the Limnoria. The

special object of the notice was, however, to show that timber subjected to preservative processes did not long resist the attacks of the Limnoria, and, more especially, that thoroughly creosoted timber is readily perforated by it, and subsequent experience has fully shown that these statements were correct.

In that notice I also said that the timber known as Greenheart has the valuable property of resisting the attack of the Limnoria, a statement which occurs in many works on Engineering and Botany, and has hitherto been universally believed to be correct. Recent experience, however, has satisfied me that this conclusion, if not absolutely incorrect, requires considerable qualification, and the object of the present notice is to communicate some facts which have been ascertained since the date of my former notice to the Society.

The Bebeeru or Greenheart tree, as is well known, is a native of British Guiana belonging to the order Lauraceæ, and its bark produces sulphate of bebeerine, which is used medicinally as a tonic.

The colour of the timber, as imported and used in engineering works, is generally light olive-green (hence its English name), with occasional darker shades approaching to brown. It can readily be got in logs of from 40 to 50 feet in length, and 10 or 15 inches square. The timber, as sent to this country, has very rarely any sapwood; the logs are seldom straight grown; and the wood, which is hard and close grained, is extremely difficult to dress owing to its tendency to split when cut up into deals or slabs. Its specific gravity is high; its weight being about 50 lbs. per cubic foot, while that of the best Memel does not exceed 30 lbs.

Independently of its supposed exemption from the ravages of the Limnoria, the fact that the breaking strength of greenheart, as compared with Memel, is as 1 to 1·51, renders it very suitable for many engineering works, and particularly for staging in situations of great exposure. It was, I believe, for the first time employed for staging at Wick Bay, where logs of pine could not withstand the waves; and it was on removing the temporary greenheart staging, that had been in use from two to four years at Wick, that I first became fully aware that the Limnoria would perforate that timber. Some of these logs were found to have been attacked by the Limnoria throughout the whole surface, extending from about low-

water mark to the bottom. This discovery caused no little surprise and regret, as engineers had always looked on greenheart as proof against destruction by marine insects; but being the first, and it was hoped perhaps an isolated instance, I did not consider it necessary at once to record the fact.

I have since, however, received a specimen of timber taken from one of the piles in the steamboat pier at Salen, in the Sound of Mull, which was erected four years ago, the main piles being made of sound greenheart, and I find that in this locality also the Limnoria has commenced to perforate the timber.

In both of these instances sufficient time has not elapsed to allow the wasting to make great progress, but in both cases the perforations have penetrated into what is unquestionably sound fresh timber; and, therefore, this result conflicts with certain other experiments, such as those made at the Bell Rock, where the greenheart remained nearly sound after nineteen years' exposure.

The joint paper of Dr Maclagan and Dr Gamgee on greenheart in the "Society's Transactions" states that by subjecting greenheart *wood* to a process identical with that used for the extraction of sulphate of bebeerine from the *bark*, a product is obtained possessed of an intensely bitter taste, and not differing perceptibly from the sulphate of bebeerine. This may account for wounds produced by a splinter of greenheart not readily healing.

I am also disposed to think that it is to the existence of this alkaloid in the timber, and not to its hardness, that its undoubted power of withstanding in certain cases and for a certain time the action of the Limnoria is due, and it would be interesting to discover whether the wasted portions of greenheart at Wick and Salen produced bebëerine in a smaller degree as compared with sound timber. It is possible, as suggested by Sir Robert Christison, that long protracted immersion in sea-water may so counteract the preservative principle due to the bebeerine in the timber as to render it open to attack. It is also possible that the greenheart now imported in such large quantities has degenerated like the "Crown Memel," which, it is well known, cannot be procured of the same high quality as formerly. Change of soil, moreover, affects the growth of trees, and is perhaps sufficient to account for the great variations in the quality of foreign grown timber.

In any view of the case, however, it seems necessary, in connection with my former notice, to make known the fact that greenheart *as now imported*, and generally used in marine works, is not, as was hitherto supposed to be the case, wholly proof against the ravages of the *Limnoria terebrans*, suggesting, perhaps, increased care in its selection, although I believe it must still be regarded as the most durable timber that can be employed in such works. It is almost unnecessary to add that these observations refer to localities where the timber is exposed to what may be termed *sea-water*, and not to situations where, from admixture of fresh water or other causes, the ravages of the Limnoria are greatly mitigated or altogether unknown.

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PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VIII. 1873–74. No. 87.

Ninety-First Session.

Monday, 24th November 1873.

Sir ROBERT CHRISTISON, Bart., President, in the Chair.

The following Council were elected:—

President.
Sir WILLIAM THOMSON, Knt., LL.D.

Honorary Vice-Presidents.
His Grace the DUKE of ARGYLL.
Sir ROBERT CHRISTISON, Bart., M.D.

Vice-Presidents.

Principal Sir Alex. Grant, Bart.
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Rev. W. Lindsay Alexander, D.D.
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General Secretary—Dr John Hutton Balfour.

Secretaries to Ordinary Meetings.
Professor Tait.
Professor Turner.

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Councillors.

Professor A. Dickson.
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Rev. Thomas Brown.
James Dewar, Esq.
Professor Lister.
George Robertson, Esq.
Captain T. P. White.
The Hon. Lord Neaves
The Right Rev. Bishop Cotterill.
Professor A. Crum Brown.
Dr Arthur Mitchell.
George Forbes, Esq.

Monday, 1st December 1873.

SIR ROBERT CHRISTISON, Bart., Honorary Vice-President, in the Chair.

The following Communications were read:—

1. Laboratory Notes. By Professor Tait.

1. First Approximation to a Thermo-electric Diagram.

(This Paper will appear in the Transactions of the Society.)

2. On the Flow of Water through Fine Tubes.

Dr Matthews Duncan recently asked me whether the flow of blood under given pressure would be affected by a considerable change of *form* of the section of a small vein or artery. It appeared obvious, from the well-known experiments of Poiseuille (which show that when the bore of a capillary tube of circular section is sufficiently small, the flow through it is as the fourth power of the diameter), that the flow through a capillary tube of elliptical section must be less than that through a circular tube of equal sectional area. The accepted theory of fluid friction might enable us to obtain a solution for an elliptic tube, but the assumptions requisite for its deduction appear extremely unlikely to be fulfilled in practice, so that I asked Messrs C. G. Knott and C. M. Smith to make some direct experimental comparisons between various circular and elliptic tubes, specially drawn for the purpose, and of the same material. The present preliminary experiments unfortunately, refer only to tubes the smallest of which has nearly the bore of the largest of those used by Poiseuille.

The tubes were carefully calibrated and the worst rejected. A length of twenty inches was cut from the most uniform portion of each of the selected tubes, and the axes of the section (when elliptical) were carefully measured at each end. This determination was checked by weighing the column of mercury employed for calibration. Water, at a fixed temperature, was drawn under

fixed pressure, for a given time, through each, and its quantity measured. The following are the experimental results:—

Section.	Weight of 1 inch Hg.	Efflux per min.	T.	P.	Axes of sections in hundredths of an inch.		Ratio of axes.	Cal. weight of 1 inch. Hg.
Elliptic.		Cub. in.	C.	In.				
I.	·249	3·30	9·4	23·65	8·5 / 8·25	1·5 / 1·8	5·073	·238
II.	·357	7·87	9·5	23·65	9· / 9·2	2·3 / 2·3	3·956	·359
III.	·441	12·	10·	23·57	9·4 / 10·1	2·6 / 2·7	3·679	·445
IV.	·685	21·77	10·	23·57	12·75 / 13·	3·1 / 3·2	4·087	·683
Circular.					Diameters of ends.			
I.	·223	6·16	10·	23·1	3·6	3·6	...	·223
II.	·301	9·62	10·	23·1	4·1	4·3	...	·304
III.	·357	11·81	10·	23·1	4·4	4·5	...	·341
IV.	·646	22·6	9·9	23·1	6·2	6·2	...	·661
V.	1·213	52·8	11·5	23·2	8·3	8·6	...	1·229
VI.	1·632	67·	11·	23·2	9·6	9·6	...	1·587

The two last were added with a view of finding the effect of still farther increasing the section. A comparison of the first and second groups of four shows the very considerable effect of the elliptic form in diminishing the rate of flow.

2. Note on the Transformation of Double and Triple Integrals. By Professor Tait.

1. If we have two equations of the form

$$f(u, v, \xi, \eta) = 0,$$
$$F(u, v, \xi, \eta) = 0,$$

u and v are given as functions of ξ and η, or *vice versâ*. Here either u and v, or ξ and η, may be the ordinary Cartesian x and y, or any given functions of them.

Now, if we write with Hamilton, since we are dealing with two independent variables only,

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy},$$

we have

$$\nabla = \nabla u\frac{d}{du} + \nabla v\frac{d}{dv} = \nabla\xi\frac{d}{d\xi} + \nabla\eta\frac{d}{d\eta} \quad . \quad . \quad . \quad . \quad (1)$$

The proof may be easily given in a Cartesian form by operating by Si and Sj separately. For the former operation gives

$$\frac{d}{dx}=\frac{du}{dx}\frac{d}{du}+\frac{dv}{dx}\frac{d}{dv}=\frac{d\xi}{dx}\frac{d}{d\xi}+\frac{d\eta}{dx}\frac{d}{d\eta},$$

equations manifestly true.

2. Now, the elementary area included by the curves u, $u+\delta u$, v, $v+\delta v$, is easily seen to be

$$\frac{\delta u\delta v}{TV\nabla u\,\nabla v}.$$

Hence we have the following transformations of a double integral extended over a given area :—

$$\iint Pdxdy=\iint P\frac{dudv}{TV\nabla u\nabla v}=\iint P\frac{d\xi d\eta}{TV\nabla\xi\nabla\eta}.$$

But by (1) we see at once that

$$TV\nabla\xi\,\nabla\eta=\begin{vmatrix}\frac{d\xi}{du}, & \frac{d\xi}{dv}\\ \frac{d\eta}{du}, & \frac{d\eta}{dv}\end{vmatrix}TV\nabla u\nabla v,$$

whence, of course, the general proposition

$$\begin{vmatrix}\frac{d\xi}{du}, & \frac{d\xi}{dv}\\ \frac{d\eta}{du}, & \frac{d\eta}{dv}\end{vmatrix}\begin{vmatrix}\frac{du}{d\xi}, & \frac{dv}{d\xi}\\ \frac{du}{d\eta}, & \frac{dv}{d\eta}\end{vmatrix}=1,$$

and the common transformation

$$\iint Pdxdy=\iint P\begin{vmatrix}\frac{dx}{du}, & \frac{dx}{dv}\\ \frac{dy}{du}, & \frac{dy}{dv}\end{vmatrix}dudv.$$

3. Dealing with triple integrals, ∇ takes the ordinary Hamiltonian form, and an additional term is added to each of the members of (1), which thus at once gives us the mode of introducing ∇ into any system of curvilinear cö-ordinates.

The element of volume included by the surfaces u, $u+\delta u$, v, $v+\delta v$, w, $w+\delta w$, is easily seen to be expressed by

$$-\frac{\delta u\delta v\delta w}{S.\nabla u\nabla v\nabla w}.$$

Hence we have the following—

$$\iiint \mathrm{P}dxdydz = -\iiint \mathrm{P}\frac{dudvdw}{S.\nabla u\nabla v\nabla w} = -\iiint \mathrm{P}\frac{d\xi d\eta d\zeta}{S.\nabla\xi\nabla\eta\nabla\zeta}.$$

From these we have, besides the more complex transformation from u, v, w, to ξ, η, ζ, the common one

$$\iiint \mathrm{P}dxdydz = -\iiint \mathrm{P}\begin{vmatrix} \frac{dx}{du}, & \frac{dx}{dv}, & \frac{dx}{dw} \\ \frac{dy}{du}, & \frac{dy}{dv}, & \frac{dy}{dw} \\ \frac{dz}{du}, & \frac{dz}{dv}, & \frac{dz}{dw} \end{vmatrix} dudvdw,$$

and also the general theorem

$$\begin{vmatrix} \frac{d\xi}{du}, & \frac{d\xi}{dv}, & \frac{d\xi}{dw} \\ \frac{d\eta}{du}, & \frac{d\eta}{dv}, & \frac{d\eta}{dw} \\ \frac{d\zeta}{du}, & \frac{d\zeta}{dv}, & \frac{d\zeta}{dw} \end{vmatrix} \begin{vmatrix} \frac{du}{d\xi}, & \frac{du}{d\eta}, & \frac{du}{d\zeta} \\ \frac{dv}{d\xi}, & \frac{dv}{d\eta}, & \frac{dv}{d\zeta} \\ \frac{dw}{d\xi}, & \frac{dw}{d\eta}, & \frac{dw}{d\zeta} \end{vmatrix} = 1.$$

3. On the Physiological Action of Ozone. By James Dewar, Esq., Lecturer on Chemistry, and John G. M'Kendrick, M.D., Physiological Laboratory, University of Edinburgh.

A systematic investigation into the physiological action of ozone, so far as we are aware, has never been undertaken. Isolated observations have been made by many while engaged in the examination of its physical and chemical properties, which have chiefly tended to show that it acts as an irritant on the mucous membrane of the respiratory tract, and they have also observed the peculiar odour which it excites by its effect on the organ of smell, from which the name *ozone* originated. Beyond this little has been attempted.

Schönbein, indeed, showed * that a mouse imprisoned in an atmosphere of ozone died in about five minutes. From meteorological data, this observer also stated that the quantity of ozone in the atmosphere and the prevalence of epidemic diseases were in an inverse relation to each other both as to time and locality. This statement has probably given rise to the popular opinion that ozone not only acts as a powerful oxidising agent of decaying animal or vegetable matters, but also that it has a specific action on the animal body.

With the view of determining what action ozone exerts on the body, we commenced a series of experimental observations, which we now beg to lay before the Society.

1. *Mode of producing the Ozone* (see fig.).—The ozone in the following experiments was made by passing a current of dry air or

Description of Figure.—*a*, glass chamber for reception of the animal; *b*, gasometer; the current of air or gas passed from right to left of diagram; *c* (to the right), bulb-tube containing sulphuric acid; *c* (to the left), bulb-tube containing caustic potash or water; *d*, U tube; *e*, wire from — pole of induction coil continuous with platinum wire within the U tube; *f*, wire from + pole of induction coil continuous with copper wire coiled round U tube.

oxygen from a gasometer (*b*) through a narrow glass tube, bent for convenience like the letter U (*d*), about 3 feet in length, and containing a platinum wire 2 feet in length, which had been inserted

* British Association Reports, 1848.

into the interior of the tube, and one end (e) of which communicated with the outside through the wall of the tube. Round the whole external surface of this U-shaped tube a spiral of copper wire was coiled, and the induction current from a coil giving ½-inch sparks was passed between the external copper (f) to the internal platinum wire (e), so as to have the platinum wire in the interior of the tube as the negative pole. After the current of gas was ozonised by the passage of the induction current, it was washed by passing through a bulb-tube (c to the left of the U tube) containing caustic potash when air was employed, or water when pure oxygen was used, in order to eliminate any traces of nitrous and nitric acids. To the right of the U tube another bulb-tube (c) was placed containing pure sulphuric acid, for removing aqueous vapour from the air, or gas passed through it. By means of the gasometer, the volume of gas passing through the apparatus could be ascertained.

2. *Method of Experiment.*—It was necessary, in the first place, to determine the action of ozone on the living animal imprisoned in an atmosphere containing a large proportion of ozone; and, in the second, to determine what action, if any, it exerted on the individual living tissues of the body.

Observations were made on frogs, birds, mice, rabbits, and on ourselves.

Frogs.—Numerous experiments were made on frogs, and the general effect on these animals is as follows:—About thirty seconds after introducing the animal into the chamber, through which a steady current of ozonised air was passing, the animal manifested symptoms of distress. The eyeballs were retracted, so as to be deeply sunk in the orbits, and the eyelids were firmly closed. It rubbed its nose occasionally with its fore paws. At first somewhat restless, the frog became lethargic, and the movements of respiration were reduced, both in frequency and force, to at least one-half the normal amount. On pushing the frog with a wire it might be excited to move, but usually it remained motionless. The position of the animal was peculiar—the neck arched, the head flattened, and it remained in a crouching attitude. This condition of lethargy has been observed to continue during a period of an hour and a half, at the end of which time the animal died. When

common air was introduced into the chamber instead of ozonised air, or if the frog was taken out of the chamber, it quickly recovered. These effects may be seen in the following experiment:—

A large healthy frog was introduced into the air-chamber, through which a current of air was passing sufficient to fill a litre flask in three minutes. At the end of two minutes, the respirations were 96 per minute. The induction machine was then set to work, so as to mix ozone with the air, the current passing through the chamber at the same rate. In half a minute the eyes were affected, and the respirations were reduced to 8 per minute. At the end of six minutes, the animal was quite motionless, and the respiratory movements had entirely ceased. Pure air was then introduced. In half a minute, there was a slight respiratory movement, and in eight minutes, the respirations numbered 85 per minute. At the end of other twelve minutes, ozone was again turned on, with the same results. The animal in this experiment was then subjected to atmospheres of common air and air mixed with ozone alternately, each period of immersion in the atmosphere consisting of ten minutes, with invariably the same effect. At the end of two hours it was removed from the chamber, and recovered.

In the case of the frog which died after being exposed to an atmosphere of ozonised air for an hour and a half, the heart was found pulsating after systemic death. It was full of dark-coloured blood. The lungs were slightly congested. In every part of the body the blood was in a venous condition.

In two experiments, frogs were exposed to the action, not of air mixed with ozone, but to a stream of oxygen mixed with ozone, and the results were somewhat different from those just narrated. The effects were not so well marked. When a frog was introduced into an atmosphere of pure oxygen, the animal was lively and vivacious, the eyes were wide open, and the respiratory movements were greatly accelerated. But when the oxygen contained a considerable quantity of ozone, the eyes were closed, the respiratory movements did not entirely cease, but were reduced from 100 or 110 to 8 or 12 per minute, and the creature was in a dormant condition. After exposure for a period of one hour, the web and the skin assumed a purple hue. After keeping the animal in such an atmosphere for 1¾ hour, it was in the same condition.

Birds.—A green linnet was put into the chamber, supplied with a strong current of air. At the end of five minutes, after the bird had become quiet, the respirations were 50 per minute. The air was then ozonised. In thirty seconds, the eyes were closed; in one minute, the respirations were reduced to 30 per minute; four minutes thereafter, the respiration was slow and gasping, and the number of movements 15 per minute; and in ten minutes, that is, fifteen and a half minutes after the introduction of ozonised air, the bird was dead. On opening the body, there was venous congestion of all the viscera. The lungs were of a dark purple colour, and showed a mottled appearance. The heart was still pulsating feebly. It was full of venous blood. The brain was pale. The blood corpuscles, when examined microscopically, were normal.

Mammals.—Several experiments were made on white mice and rabbits. With regard to mice, the general effects will be understood by detailing one experiment. A full-grown and apparently healthy white mouse was introduced into a vessel through which a stream of air was passing at the rate of 8 cubic inches per minute. Five minutes thereafter, the animal was evidently at ease, and the respirations were 136 per minute. The air was then ozonised. One minute after, the respirations were somewhat slower, but could not be readily counted, owing to the animal moving uneasily about and rubbing its nose with its fore paws. In four minutes from the time of introduction of the ozone, the respirations were 32 in a minute. The mouse now rested quietly, occasionally yawned, and when touched by a wire, moved, but always in such a direction as to place its head away as far as possible from the stream of ozonised air. At the end of fifteen minutes, the animal became excited, ran rapidly backwards and forwards, and then had a convulsive attack. It died, much convulsed, nineteen minutes after the introduction of the ozone. The body was colder than natural. There was venous congestion of all the abdominal viscera. The heart was still feebly pulsating, and the right auricle and ventricle were full of venous blood. The left side of the heart contained a small quantity of venous blood. The sinuses of the brain were full of dark blood, and the surface and base of the brain was traversed by vessels containing dark-coloured blood.

Two experiments were also made upon mice, in which, instead

of being supplied with ozonised air, they received ozonised oxygen. When a mouse breathed an atmosphere of pure oxygen, it became exceedingly active in its movements. It ran about examining every part of its prison, and breathed with such rapidity as to make it impossible to count the number of respirations taken during a minute. When the oxygen was ozonised, the mouse quickly showed the usual phenomena of the closed eyes and the reduction of the number of respirations, but it lived for a much longer period than in ozonised air. Instead of dying at the end of fifteen or twenty minutes after the introduction of the ozonised atmosphere, it lived for thirty-five or forty minutes. The number of respirations per minute became smaller, and the animal died in severe general convulsions. The blood, when examined quickly after death, has been found venous in all parts of the body. In both experiments, the temperature of the body was found to be much reduced.

As the reduced temperature of the body in these experiments might have been owing to the current of gas passing quickly over the bodies of the animals, two experiments were made, in which the glass air-chamber was immersed in a water-bath kept at a temperature of 30° C. The animals were supplied with atmosphere at the rate of 13 cubic inches per minute. The general results were the same as in the experiments made without the water-bath, but the temperature of the body on death was still below the normal.

Various experiments were also made on rabbits, with the same general results as in the case of mice. There was evident irritation of the eyes, causing closure of the lids, and the exudation from between their margins of a whitish fluid, probably lachrymal secretion. The respirations were reduced in number from 100 or 110 to from 36 to 30 per minute. In one experiment, only the head of the rabbit was introduced into a glass vessel, into which the stream of ozonised oxygen was transmitted so as to allow the experimenter to count by touch the number per minute of the pulsations of the heart. The result was, that immediately on the introduction of ozone the number of pulsations was much diminished, and the force of the contractions of the heart was so enfeebled that it could not be felt through the wall of the thorax. Still, in the bodies of rabbits killed in an atmosphere of ozonised air, or of

ozonised oxygen, the heart was found pulsating, and, as in the other cases, engorged with venous blood.

On breathing an atmosphere of ozonised oxygen ourselves, the chief effects observed were a suffocating feeling in the chest, a tendency to breathe slowly, an irritation of the back of the throat and of the glottis, and a tingling sensation, referred to the skin of the face and the conjunctivæ. The pulse became feebler. After breathing it as long as it was judicious to do, say for five or eight minutes, the suffocating feeling became stronger, and we were obliged to desist. The experiment was followed by violent irritating cough and sneezing, and for five or six hours thereafter by a sensation of rawness in the throat and air-passages.

The action of ozone on several of the chief physiological systems, and on various tissues, was also examined.

1. *On the Circulation.*—By a suitable apparatus, a frog was imprisoned in a chamber through which a stream of ozonised air, or of ozonised oxygen, passed, while at the same time the web was so placed under a microscope that the circulation in the smaller vessels and capillaries could be readily observed. The result was negative, inasmuch as no appreciable acceleration or retardation of the current of the circulation was seen.

2. *On the Reflex Action of the Spinal Cord.*—This function was not affected to any appreciable degree.

3. *On Muscular Contractility.*—By means of a myographion, the work done by the gastrocnemii of frogs, subjected to the action of ozone, was noted. The muscles were stimulated by a single opening or closing induction shock produced by Du Bois Reymond's apparatus and a Daniell's cell. The result was that the contractility and work-power of the muscle were found unaffected, as far as could be appreciated.

4. *On the Blood.*—When a thin layer of human blood on a slide is exposed to the action of ozone, the coloured corpuscles become paler, lose their definite outline, and if exposed for a period of five or ten minutes to the action of the current, they are dissolved, and a mass of molecular material is seen. The coloured corpuscles of the frog show, after the action of ozone, the formation of a nucleus. By prolonged exposure many of the nuclei apparently pass out of the substance of the corpuscle, numerous free nuclei are seen, and

some in the act of separating from the corpuscle have been observed. The colourless corpuscles are contracted into globular masses after the action of ozone. The general effects resemble those produced by a weak acid, such as very dilute acetic acid or a stream of carbonic acid.

5. *On Ciliary Motion.*—When the cilia of the common mussel (*Mytilus edulis*) were exposed to the action of ozone, while bathed in the fluid contained in the shell (sea-water), no effect was observed. This is owing to the protection to the cilia afforded by the water. If a very small amount of water covered the cilia, their action was at once arrested.

From the preceding experiments the following general facts may be stated:—

1. The inhalation of an atmosphere highly charged with ozone diminishes the number of respirations per minute.

2. The pulsations of the heart are reduced in strength, and this organ is found beating feebly after the death of the animal.

3. The blood is always found in a venous condition in all parts of the body, both in cases of death in an atmosphere of ozonised air and of ozonised oxygen.

4. Ozone exercises a destructive action on the living animal tissues if brought into immediate contact with them; but it does not affect them so readily if they are covered by a layer of fluid.

5. Ozone acts as an irritant to the mucous membrane of the nostrils and air-passages, as all observers have previously remarked.

At the present state of this inquiry, it would be premature to generalise regarding the relation between physiological action and the chemical properties of ozone; but we can hardly avoid pointing out that oxygen in this altered condition ($O_3 = 24$) is slightly denser than carbonic acid ($CO_2 = 22$), and that, although the chemical activity of the substance is much increased, yet when inhaled into the lungs, it must retard greatly the rate of diffusion of carbonic acid from the blood, which accounts for the venous character of that fluid after death. If, however, the physiological effect of ozone on respiration were merely due to its greater density, then we would expect its behaviour to be analogous to that of an atmosphere highly charged with carbonic acid. This has been found to be the case, more especially as regards the diminished number of respirations per minute, and the appearance of the blood after death.

If, however, this analogy were perfect, we would anticipate that the action of oxygen, partially ozonised, would not have produced death, as the amount of ozone in these experiments certainly did not exceed 10 per cent. As it was, all we have observed is that the animal only lives a somewhat longer time in ozonised oxygen than in ozonised air. We are thus induced to regard ozone as having some specific action on the blood, or in the reflex nervous arrangements of respiration, that future experiments may elucidate.

4. On a Compound formed by the addition of Bromacetic Acid to Sulphide of Methyl, and on some of its Derivatives. By Professor Crum Brown and Dr E. A. Letts.

(*Abstract.*)

The sulphine compounds discovered by v. Oefele, indicate that, notwithstanding the difference of atomicity, there exists an analogy between sulphur and nitrogen, these compounds corresponding to the salts of the ammonium bases. not only in chemical properties but also in physiological action.*

The research, the results of which are communicated in this paper, was undertaken with the view of examining this analogy in some other directions.

It seemed reasonable to suppose that, as the nitrile bases, such as trimethylamine and strychnia unite with chloracetic acid to form compounds such as hydrochlorate of betaine and of glycolyl-strychnia, the sulphides of the alcohol radicals should act in a similar way. Experiments show that this is the case—bromacetic acid acting readily on sulphide of methyl to form a beautifully crystallised compound to which the authors give the name of hydrobromate of methyl-thetine. Analyses proved this substance to have the composition corresponding to the formula $C_4H_9SBrO_2$ which is that of the sulphur analogue of the hydrobromate of betaine.

$$\underset{\text{hydrobromate of methylthetine.}}{\mathrm{Br}-\overset{\overset{(CH_3)_2}{\|}}{\mathrm{S}}-\mathrm{CH_2}-\mathrm{COOH}}\ ;\ \underset{\text{hydrobromate of betaine.}}{\mathrm{Br}-\overset{\overset{(CH_3)_3}{|||}}{\mathrm{N}}-\mathrm{CH_2}-\mathrm{COOH}}$$

This view of its constitution is confirmed by its reactions.

* Brown and Fraser, "Proc. Royal Soc. Edin.," March 4th, 1872.

In addition to this substance, which served as a starting-point for the research, the nitrate, the chloroplatinate, the chloraurate, the bromaurate, and compounds formed by the action of the hydrobromate on the oxides of mercury, copper, and lead, on ammonia and on ethylate of sodium were examined.

Corresponding addition products of sulphide of ethyl were also prepared, but owing to the extremely deliquescent character of the hydrobromate of ethyl-thetine, attention was chiefly devoted to the derivatives of the methyl compound.

Iodacetic ether does not form an addition product with sulphide of methyl. The reaction here takes a different direction, free iodine and iodide of trimethylsulphine being produced. The authors are engaged in the investigation of this reaction, and also of the products of the oxidation of the thetine compounds.

5. Note on the Various Possible Expressions for the Force Exerted by an Element of one Linear Conductor on an Element of another. By Professor Tait.

In the *Quarterly Mathematical Journal* for 1860, I gave a quaternion process for obtaining in a very simple manner, from Ampère's experimental data, his well-known expression for the mutual action between two elements of currents. As one of the data the assumption was made, after Ampère, that the action is a force whose direction is that of the line joining the middle points of the elements, *i.e.*, it was assumed that the necessary equality of action and reaction holds, not merely for two closed circuits but, for each pair of elements of these circuits. I promised in that paper to publish a more general investigation, in which no such assumption should be made; but I was prevented from doing this by having seen a reference to a memoir by Cellerier, in which it was stated that such an investigation had been given. I did not, till very recently, succeed in getting any information about that memoir, none of which seems indeed to have been printed except a very brief extract in the *Comptes Rendus* for 1850, vol. xxx., giving no details: but the subject was recalled to my memory by Clerk-Maxwell's *Treatise on Electricity, &c.*, in which there is an investi-

gation of the possible expressions for the forces which satisfy Ampère's data without necessarily satisfying his assumption. Both of these authors make the undetermined part of the expression depend upon a single arbitrary function. My investigation leads to two. The question is one of comparatively little physical importance, but I give this investigation for its extreme simplicity.

The following is, as nearly as I can recollect, my original process, which has, at least at first sight, nothing in common with that of Clerk-Maxwell.

1. Ampère's data for closed currents are briefly as follows:—

I. Reversal of either current reverses the mutual effect.

II. The effect of a sinuous or zig-zag current is the same as that of a straight or continuously curved one, from which it nowhere deviates much.

III. No closed current can set in motion a portion of a circular conductor movable about an axis through its centre, and perpendicular to its plane.

IV. In similar systems, traversed by equal currents, the forces are equal.

2. First, let us investigate the expression for the *force* exerted by one element on another.

Let α be the vector joining the elements α_1, α', of two circuits; then, by I., II., the action of α_1 on α' is *linear* in each of α_1, α', and may, therefore, be expressed as

$$\phi\alpha',$$

where ϕ is a linear and vector function, into each of whose constituents α_1 enters linearly.

The resolved part of this along α' is

$$S . U\alpha'\phi\alpha',$$

and, by III., this must be a complete differential as regards the circuit of which α_1 is an element. Hence,

$$\phi\alpha' = -(S . \alpha_1\nabla)\psi\alpha' + V\alpha'\chi\alpha_1,$$

where ψ and χ are linear and vector functions whose constituents

involve α only. That this is the case follows from the fact that $\phi\alpha'$ is homogeneous and linear in each of α_1, α'. It farther follows, from IV., that the part of $\phi\alpha'$ which does not disappear after integration round each of the closed circuits is of no dimensions in $T\alpha$, $T\alpha'$, $T\alpha_1$. Hence χ is of -2 dimensions in $T\alpha$, and thus

$$\chi\alpha_1 = \frac{p\alpha S\alpha\alpha_1}{T\alpha^4} + \frac{q\alpha_1}{T\alpha^2} + \frac{rV\alpha\alpha_1}{T\alpha^3}$$

where p, q, r are numbers.

Hence we have

$$\phi\alpha' = -S(\alpha_1\nabla)\psi\alpha' + \frac{pV\alpha\alpha' S\alpha\alpha_1}{T\alpha^4} + \frac{qV\alpha'\alpha_1}{T\alpha^2} + \frac{rV.\alpha' V\alpha\alpha_1}{T\alpha^3}.$$

Change the sign of α in this, and interchange α' and α_1, and we get the action of α' on α_1. This, with α' and α_1 again interchanged, and the sign of the whole changed, should reproduce the original expression—since the effect depends on the relative, not the absolute, positions of α, α_1, α'. This gives at once,

$$p = 0 \ , \ q = 0,$$

and

$$\phi\alpha' = -S(\alpha_1\nabla)\psi\alpha' + \frac{rV.\alpha' V\alpha\alpha_1}{T\alpha^3},$$

with the condition that the first term changes its sign with α, and thus that

$$\psi\alpha' = \alpha S\alpha\alpha' F(T\alpha) + \alpha' F(T\alpha),$$

which, by change of F, may be written

$$= \alpha S(\alpha'\nabla) f(T\alpha) + \alpha' F(T\alpha),$$

where f and F are any scalar functions whatever.

Hence

$$\phi\alpha' = -S(\alpha_1\nabla)[\alpha S(\alpha'\nabla) f(T\alpha) + \alpha' F(T\alpha)] + \frac{rV.\alpha' V\alpha\alpha_1}{T\alpha^3}.$$

which is the general expression required.

3. The simplest possible form for the action of one current-element on another is, therefore,

$$\phi\alpha' = \frac{rV.\alpha' V\alpha\alpha_1}{T\alpha^3}.$$

Here it is to be observed that Ampère's *directrice* for the circuit α_1 is

$$\theta = \int \frac{V\alpha\alpha_1}{T\alpha^3},$$

the integral extending round the circuit; so that, finally,

$$\phi\alpha' = -rS\alpha_1 \nabla V\alpha'\theta.$$

4. We may obtain from the general expression above the absolutely symmetrical form,

$$\frac{rV.\alpha'\alpha\alpha_1}{T\alpha^3},$$

if we assume

$$f(T\alpha) = \text{const}, \ F(T\alpha) = \frac{r}{T\alpha}.$$

Here the action of α' on α_1 is parallel and equal to that of α_1 on α'. The forces, in fact, form a couple, for α is to be taken negatively for the second—and their common direction is the vector drawn to the corner a of a spherical triangle abc, whose sides ab, bc, ca in order are bisected by the extremities of the vectors $U\alpha'$, $U\alpha$, $U\alpha_1$. Compare Hamilton's *Lectures on Quaternions*, §§ 223–227.

5. To obtain Ampère's form for the effect of one element on another write, in the general formula above,

$$f(T\alpha) = \frac{r}{T\alpha}, \ F(T\alpha) = 0,$$

and we have

$$\begin{aligned}
\frac{1}{r}\phi\alpha' &= -S\alpha_1\nabla\left[-\frac{\alpha S\alpha\alpha'}{T\alpha^3}\right] + \frac{V.\alpha' V\alpha\alpha_1}{T\alpha^3}, \\
&= -\frac{\alpha_1 S\alpha\alpha'}{T\alpha^3} - \frac{\alpha S\alpha_1\alpha'}{T\alpha^3} - \frac{3\alpha S\alpha\alpha' S\alpha\alpha_1}{T\alpha^5} + \frac{V.\alpha' V\alpha\alpha_1}{T\alpha^3}, \\
&= +\frac{2\alpha}{T\alpha^5}\left(\alpha^2 S\alpha_1\alpha' - \frac{3}{2} S\alpha\alpha' S\alpha\alpha_1\right), \\
&= -\frac{2\alpha}{T\alpha^5}\left(S.V\alpha\alpha' V\alpha\alpha_1 + \frac{1}{2} S\alpha\alpha' S\alpha\alpha_1\right),
\end{aligned}$$

which are the usual forms.

6. The remainder of the expression, containing the arbitrary terms, is of course still of the form

$$-S(\alpha_1\nabla)\left[\alpha S(\alpha'\nabla) f(T\alpha) + \alpha' F(T\alpha)\right].$$

In the ordinary notation this expresses a force whose components are proportional to

(1.) Along α $\qquad -r\dfrac{d^2f}{ds_1 ds'}$,

(Note that, in *this* expression, r is the distance between the elements.)

(2.) Parallel to α' $\qquad \dfrac{dF}{ds_1}$,

(3.) Parallel to α_1 $\qquad -\dfrac{df}{ds'}$.

If we assume $f = F = -Q$, we obtain the result given by Clerk-Maxwell (*Electricity and Magnetism*, § 525), which differs from the above only because he assumes that the force exerted by one element on another when the first is parallel and the second perpendicular to the line joining them is *equal* to that exerted when the first is perpendicular and the second parallel to that line.

7. What precedes is, of course, only a particular case of the following interesting problem:—

Required the most general expression for the mutual action of two rectilinear elements, each of which has dipolar symmetry in the direction of its length, and which may be resolved and compounded according to the usual kinematical law.

The data involved in this statement are equivalent to I. and II. of Ampère's data above quoted. Hence, keeping the same notation as in § 2 above, the force exerted by α_1 on α' must be expressible as

$$\phi\alpha'$$

where ϕ is a linear and vector function, whose constituents are linear and homogeneous in α_1; and, besides, involve only α.

By interchanging α_1 and α', and changing the sign of α, we get the force exerted by α' on α_1. If in this we again interchange α_1 and α', and change the sign of the whole, we must obviously reproduce $\phi\alpha'$. Hence we must have $\phi\alpha'$ changing its sign with α, or

$$\phi\alpha' = \mathrm{P}\alpha S\alpha_1\alpha' + \mathrm{Q}\alpha S\alpha\alpha_1 S\alpha\alpha' + \mathrm{R}\alpha_1 S\alpha\alpha' + \mathbf{R}\alpha' S\alpha\alpha_1$$

where P, Q, R, $\mathbf{R}$ are functions of $T\alpha$ only.

8. The vector *couple* exerted by α_1 on α' must obviously be expressible in the form

$$V.\alpha'\varpi\alpha_1 ,$$

where ϖ is a new linear and vector function depending on α alone. Hence its most general form is

$$\varpi\alpha_1 = P\alpha_1 + Q\alpha S\alpha\alpha_1 ,$$

where P and Q are functions of $T\alpha$ only. The form of these functions, whether in the expression for the force or for the couple, depends on the special data for each particular case. Symmetry shows that there is no term such as

$$RV\alpha\alpha_1 .$$

9. As an example, let α_1 and α' be elements of solenoids or of uniformly and linearly magnetised wires, it is obvious that, as a closed solenoid or ring-magnet exerts no external action,

$$\phi\alpha' = - S\alpha_1\nabla . \psi\alpha' .$$

Thus we have introduced a different datum in place of Ampère's No. III. But in the case of solenoids the Third Law of Newton holds—hence

$$\phi\alpha' = S\alpha_1\nabla S\alpha'\nabla . \chi\alpha ,$$

where χ is a linear and vector function, and can therefore be of no other form than

$$\alpha f(T\alpha) .$$

Now two solenoids, each extended to infinity in one direction, act on one another like two magnetic poles, so that (this being our equivalent for Ampère's datum No. IV.)

$$\chi\alpha = p \frac{\alpha}{T\alpha^3} .$$

Hence the vector force exerted by one small magnet on another is

$$pS\alpha_1\nabla S\alpha'\nabla \frac{\alpha}{T\alpha^3} .$$

10. For the couple exerted by one element of a solenoid, or of a uniformly and longitudinally magnetised wire, on another, we have of course the expression

$$V . \alpha'\varpi\alpha_1 ,$$

where ϖ is some linear and vector function.

Here, in the first place, it is obvious that

$$\varpi a_1 = - Sa_1\nabla . \frac{a}{F(Ta)};$$

for the couple vanishes for a closed circuit of which a_1 is an element, and the integral of ϖa_1 must be a linear and vector function of a alone. It is easy to see that in this case

$$F(Ta) \propto (Ta)^3 .$$

11. If, again, a_1 be an element of a solenoid, and a' an element of current, the force is

$$\phi a' = - Sa_1\nabla . \psi a' ,$$

where

$$\psi a' = Pa' + QaSaa' + RVaa' .$$

But no portion of a solenoid can produce a force on an element of current in the direction of the element, so that

$$\phi a' = V . a'\chi a_1 ,$$

so that

$$P = 0, \quad Q = 0,$$

and we have

$$\phi a' = - Sa_1\nabla(RVaa') .$$

This must be of -1 linear dimensions when we integrate for the effect of one pole of a solenoid, so that

$$R = \frac{p}{Ta^3} .$$

If the current be straight and infinite each way, its equation being

$$a = \beta + x\gamma ,$$

where

$$T\gamma = 1 \text{ and } S\beta\gamma = 0,$$

we have, for the whole force exerted on it by the pole of a solenoid, the expression

$$p\beta\gamma\int_{-\infty}^{+\infty}\frac{dx}{(T\beta^2 + x^2)^{\frac{3}{2}}} = - 2p\beta^{-1}\gamma ,$$

which agrees with known facts.

12. Similarly, for the couple produced by an element of a solenoid on an element of a current we have

$$Va'\varpi a_1 ,$$

where

$$\varpi\alpha_1 = -S\alpha_1\nabla.\psi\alpha,$$

and it is easily seen that

$$\psi\alpha = \frac{r\alpha}{T\alpha^3}.$$

13. In the case first treated, the couple exerted by one current-element on another is (§ 8 above)

$$V.\alpha'\varpi\alpha_1,$$

where, of course, $\pm\varpi\alpha_1$ are the vector forces applied at either end of α'. Hence the work done when α' changes its direction is

$$-S.\delta\alpha'\varpi\alpha_1,$$

with the condition

$$S.\alpha'\delta\alpha' = 0.$$

So far, therefore, as change of direction of α' alone is concerned, the mutual potential energy of the two elements is of the form

$$S.\alpha'\varpi\alpha_1.$$

This gives, by the expression for ϖ in § 8, the following value

$$PS\alpha'\alpha_1 + QS\alpha\alpha'S\alpha\alpha_1.$$

Hence, integrating round the circuit of which α_1 is an element, we have (*On Green's and other Allied Theorems*, § 11, *Trans.* R.S.E., 1869–70)

$$\begin{aligned}\int(PS\alpha'\alpha_1 + QS\alpha\alpha'S\alpha\alpha_1) &= \iint ds_1S.U\nu_1\nabla(P\alpha' + Q\alpha S\alpha\alpha'),\\ &= \iint ds_1S.U\nu_1\left(\frac{\alpha\alpha'P'}{T\alpha} - \alpha'\alpha Q\right),\\ &= \iint ds_1S.U\nu_1V\alpha\alpha'\Phi,\end{aligned}$$

where

$$\Phi = \frac{P'}{T\alpha} + Q.$$

Integrating this round the other circuit we have for the mutual potential energy of the two, so far as it depends on the expression above, the value

$$\begin{aligned}&\iint ds_1S.U\nu_1\int V\alpha\alpha'\Phi\\ &= -\iint ds_1S.U\nu_1\iint ds'V.V(U\nu'\nabla)\alpha\Phi\\ &= \iint ds_1\iint ds'\left\{S.U\nu_1U\nu'(2\Phi + T\alpha\Phi') + S\alpha U\nu'S\alpha U\nu_1\frac{\Phi'}{T\alpha}\right\}.\end{aligned}$$

But, by Ampère's result, that two closed circuits act on one another as two magnetic shells, it should be

$$\iint ds_1 \iint ds' S.U\nu_1 \nabla S.U\nu' \nabla \frac{1}{Ta}$$

$$= \iint ds_1 \iint ds' \left(S.U\nu_1 U\nu' \frac{1}{Ta^3} + 3 SaU\nu' SaU\nu_1 \frac{1}{Ta^5} \right).$$

Comparing, we have

$$\left. \begin{aligned} \frac{1}{Ta^3} &= 2\Phi + Ta\Phi' \\ \frac{3}{Ta^3} &= Ta\Phi' \end{aligned} \right\},$$

giving

$$\Phi = -\frac{1}{Ta^3}; \quad \Phi' = \frac{3}{Ta^4},$$

which are consistent with one another, and which lead to

$$\frac{P'}{Ta} + Q = -\frac{1}{Ta^3}.$$

Hence, if we put

$$Q = \frac{1-n}{2nTa^3},$$

we get

$$P = \frac{1+n}{2nTa},$$

and the mutual potential of two elements is of the form

$$(1+n)\frac{Sa'a_1}{Ta} + (1-n)\frac{Saa'Saa_1}{Ta^3},$$

which is the expression employed by Helmholtz in his recent paper. (*Ueber die Bewegungsgleichungen der Electricität*, *Crelle*, 1870, p. 76.)

Monday, 22d December 1873.

Sir W. THOMSON, President, in the Chair.

Professor Andrews, Hon. F.R.S.E., Vice-President of Queen's College, Belfast, gave an Address on Ozone.

Monday, 5th January 1874.

Professor Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. A new Method of Determining the Material and Thermal Diffusivities of Fluids. By Sir William Thomson.

2. Continuants—A New Special Class of Determinants. By Thomas Muir, M.A., Assistant to the Professor of Mathematics in the University of Glasgow.

1. A determinant which has the elements lying outside the principal diagonal and the two bordering minor diagonals each equal to zero, and which has the elements of one of these minor diagonals each equal to negative unity, may be called a *Continuant*. Thus

$$\begin{vmatrix} a_1 & b_1 & 0 & 0 \\ -1 & a_2 & b_2 & 0 \\ 0 & -1 & a_3 & b_3 \\ 0 & 0 & -1 & a_4 \end{vmatrix}$$

is a continuant of the fourth order.

2. A continuant is evidently a function of the elements of the principal diagonal and the variable minor diagonal, and of these alone. Let this function be denoted by K. The above continuant, for example, may then be written

$$K\begin{pmatrix} \ b_1 \ b_2 \ b_3 \ \\ a_1 \ a_2 \ a_3 \ a_4 \end{pmatrix}.$$

3. By the cyclical transposition of rows and thereafter of columns, we establish a first law of continuants, viz.:—

$$K\begin{pmatrix} b_1 \cdots\cdots b_{n-1} \\ a_1\ a_2 \cdots\cdots a_{n-1}\ a_n \end{pmatrix} = K\begin{pmatrix} b_{n-1} \cdots\cdots b_1 \\ a_n\ a_{n-1} \cdots\cdots a_2\ a_1 \end{pmatrix} \quad . \quad \text{(I.)}$$

4. By expansion of the continuant in terms of its principal minors we have

$$K\begin{pmatrix} b_1 \cdots\cdots b_{n-1} \\ a_1 a_2 \cdots a_{n-1} a_n \end{pmatrix} = a_1 K\begin{pmatrix} b_2 \cdots\cdots b_{n-1} \\ a_2 a_3 \cdots a_{n-1} a_n \end{pmatrix} + b_1 K\begin{pmatrix} b_3 \cdots b_{n-1} \\ a_3 \cdots a_{n-1} a_n \end{pmatrix} \quad \text{(II.)}$$

5. From this we see how to evaluate a continuant for special values of its elements, and also to change a continuant into the ordinary notation, *i.e.*, to free it of determinant forms. Thus,

$$K\begin{pmatrix} 4\ 6\ 8\ 9\ 7 \\ 7\ 2\ 3\ 1\ 4\ 5 \end{pmatrix}$$

would be evaluated by first evaluating $K\begin{pmatrix} 7 \\ 4\ 5 \end{pmatrix}$, thence $K\begin{pmatrix} 9\ 7 \\ 1\ 4\ 5 \end{pmatrix}$, thence $K\begin{pmatrix} 8\ 9\ 7 \\ 3\ 1\ 4\ 5 \end{pmatrix}$, and so on.

6. By means of Laplace's expansion-theorem we can establish a result which includes (II.) viz.,

$$K\begin{pmatrix} b_1 \cdots\cdots\cdots b_{n-1} \\ a_1 a_2 \cdots a_p \cdots a_{n-1} a_n \end{pmatrix} = K\begin{pmatrix} b_1 \cdots b_{p-1} \\ a_1 a_2 \cdots\cdots a_p \end{pmatrix} K\begin{pmatrix} b_{p+1} \cdots b_{n-1} \\ a_{p+1} \cdots\cdots a_n \end{pmatrix}$$

$$+ b_p K\begin{pmatrix} b_1 \cdots b_{p-2} \\ a_1 \cdots\cdots a_{p-1} \end{pmatrix} K\begin{pmatrix} b_{p+2} \cdots b_{n-1} \\ a_{p+2} \cdots\cdots a_n \end{pmatrix} \quad \text{(III.)};$$

and, using instead the present author's extension of Laplace's theorem, we arrive at a still more general proposition, viz.,

$$K\begin{pmatrix} b_1 \cdots b_{n-1} \\ a_1 a_2 \cdots\cdots a_n \end{pmatrix} K\begin{pmatrix} b_h \cdots b_{p-1} \\ a_h \cdots\cdots a_p \end{pmatrix}$$

$$= K\begin{pmatrix} b_1 \cdots b_{p-1} \\ a_1 \cdots\cdots a_p \end{pmatrix} K\begin{pmatrix} b_h \cdots b_{n-1} \\ a_h \cdots\cdots a_n \end{pmatrix}$$

$$+ (-1)^{p-h+1} b_{h-1} b_h \cdots b_p K\begin{pmatrix} b_1 \cdots b_{h-3} \\ a_1 \cdots\cdots a_{h-2} \end{pmatrix} K\begin{pmatrix} b_{p+2} \cdots b_{n-1} \\ a_{p+2} \cdots\cdots a_n \end{pmatrix} \quad \text{(IV.)},$$

where of course $h<p<n$. An important particular case is that for which $p=n-1$ and $h=2$.

7. Another result which is easily proved by induction is

$$K\begin{pmatrix} b_1 \ldots\ldots\ldots\ldots b_{n-1} \\ -a_1, -a_2 \ldots -a_{n-1}, -a_n \end{pmatrix} = (-1)^n K\begin{pmatrix} b_1 \ldots\ldots b_{n-1} \\ a_1 a_2 \ldots a_{n-1} a_n \end{pmatrix} \quad . \quad \text{(V.)}$$

8. In any continuant

$$K\begin{pmatrix} b_1 \ldots\ldots b_{n-1} \\ a_1 a_2 \ldots a_{n-1} a_n \end{pmatrix}$$

we may call $a_1 a_2 \ldots\ldots a_{n-1} a_n$ the *main diagonal*, and $b_1 b_2 \ldots\ldots b_{n-1}$ the *minor diagonal*; $a_1, a_2, \ldots\ldots, b_1, b_2, \ldots\ldots$ being known as *elements*. When each element of the minor diagonal is unity, the continuant may be called *simple*, and in writing such continuants we may agree to omit the minor diagonal, putting, for example,

$$K(a_1 a_2 a_3 \ldots\ldots a_{n-1} a_n) \text{ for } K\begin{pmatrix} 1\ 1 \ldots\ldots 1 \\ a_1\ a_2\ a_3 \ldots a_{n-1} a_n \end{pmatrix}.$$

9. If the elements of the first column of the determinant $K(1\ a_1 a_2 \ldots a_n)$ be subtracted from the corresponding elements of the second column, it will be seen that

$$K(1, a_1, a_2, \ldots a_n) = K(a_1+1, a_2, \ldots a_n) \quad . \quad \text{(VI.)}$$

10. From (II.) it is clear that

$$K(0, a_2, a_3, \ldots a_n) = K(a_3 \ldots a_n),$$

thence, with the help of (III.), we can show that

$$K(\ldots a, b, c, 0, e, f, g, \ldots) = K(\ldots a, b, c+e, f, g, \ldots) \quad \text{(VII.)},$$

and from this that

$$K(\ldots a, b, c, 0, 0, 0, e, f, \ldots) = K(\ldots a, b, c+e, f, \ldots)$$

and so, generally, when the number of consecutive zero elements is odd.

11. Similarly, from (II.)

$$K(0, 0, a_3, a_4, \ldots a_n) = K(a_3, a_4, \ldots a_n),$$

and from this, with the help of (III.), we can prove that

$$K(\dots a, b, 0, 0, e, f, \dots) = K(\dots a, b, e, f, \dots) \quad . \quad \text{(VIII.)},$$

and so, generally, when the number of consecutive zero elements is even.

12. Using the ordinary process of finding the greatest common measure of two numbers, we may establish another special property of simple continuants, viz., that, whatever $a_1, a_2, \dots$ may be,

$$K(a_1, a_2, \dots a_{n-1}, a_n)$$

is prime to

$$K(a_1, a_2, \dots a_{n-1}), \quad K(a_2, \dots a_{n-1}, a_n), \quad K(a_1 - 1, a_2, \dots a_n), \text{ and}$$
$$K(a_1, a_2, \dots a_n - 1) \, .$$

13. When both diagonals of a continuant are the same when read backwards as when read forwards, it may be called *symmetrical.*

In connection with simple symmetrical continuants, the following identities may be mentioned:—

$$K(a_1, a_2, \dots a_{n-1}, a_n, a_{n-1}, \dots a_2, a_1) = K(a_1, a_2, \dots a_{n-1})\{K(a_1, a_2, \dots a_{n-2}) + K(a_1, a_2, \dots a_n)\} \qquad \text{(IX.)}$$

$$K(a_1, a_2, \dots a_n, a_n, \dots a_2, a_1) = K(a_1, a_2, \dots a_{n-1})^2 + K(a_1, a_2, \dots a_n)^2 \qquad \text{(X.)}$$

$$a_n K(a_1, a_2, \dots a_{n-1}, a_n, a_{n-1}, \dots a_2, a_1) = K(a_1, a_2, \dots a_n)^2 - K(a_1, a_2, \dots a_{n-2})^2 \qquad \text{(XI.)}$$

$$K(a_1, a_2, \dots a_{n-1}, 2a_n, a_{n-1} \dots a_2, a_1) = 2K(a_1, a_2, \dots a_{n-1})\, K(a_1, a_2, \dots a_n) \qquad \text{(XII.)}$$

Connection between Continuants and Continued Fractions.

14. The value of the special study of this class of determinants lies in the fact that by means of them the convergents of a con-

tinued fraction are expressible in an unexpectedly simple and elegant manner. Thus—

$$x + \frac{y}{z} = \frac{xz + y}{z} = \frac{\mathrm{K}\begin{pmatrix} & y & \\ x & , & z \end{pmatrix}}{\mathrm{K}(z)};$$

$$v + \frac{w}{x +}\frac{y}{z} = v + \cfrac{w}{\cfrac{\mathrm{K}\begin{pmatrix} & y & \\ x & , & z \end{pmatrix}}{\mathrm{K}(z)}}$$

$$= \frac{v\mathrm{K}\begin{pmatrix} & y & \\ x & , & z \end{pmatrix} + w\mathrm{K}(z)}{\mathrm{K}\begin{pmatrix} & y & \\ x & , & z \end{pmatrix}}$$

$$= \frac{\mathrm{K}\begin{pmatrix} & w & & y & \\ v & , & x & , & z \end{pmatrix}}{\mathrm{K}\begin{pmatrix} & y & \\ x & , & z \end{pmatrix}};$$

and thus by induction we prove that—

$$a_1 + \frac{b_1}{a_2 +}\frac{b_2}{a_3 +} \cdots + \frac{b_{n-2}}{a_{n-1} +}\frac{b_{n-1}}{a_n} = \frac{\mathrm{K}\begin{pmatrix} & b_1 & b_2 & \ldots\ldots & b_{n-1} & \\ a_1, & a_2, & a_3 & \ldots & a_{n-1}, & a_n \end{pmatrix}}{\mathrm{K}\begin{pmatrix} & b_2 & \ldots\ldots & b_{n-1} & \\ a_2, & a_3 & \ldots & a_{n-1}, & a_n \end{pmatrix}} \quad . \quad . \quad \text{(XIII.)}$$

15. In virtue of this connection continuants will be found of the utmost aid in investigating the properties of continued fractions. The following are a few instances of this relating to those continued fractions which are expressible in the form of quadratic surds.

16. Consider the periodic continued fraction—

$$\mathrm{A} + \frac{b_1}{\underset{*}{a_1} +}\frac{b_2}{a_2 +}\frac{b_3}{a_3 +} \cdots + \frac{b_3}{a_2 +}\frac{b_2}{a_1 +}\frac{b_1}{\underset{*}{2\mathrm{A}} +} \cdots$$

where the asterisks are used like the superposed dots in the notation of decimal fractions to indicate the recurring portion or period.

Denoting it by x, we have

$$x = A + \frac{b_1}{a_1 +}\,\frac{b_2}{a_2 +}\cdots + \frac{b_2}{a_2 +}\,\frac{b_1}{a_1 +}\,\frac{b}{2A + x - A}$$

$$= \frac{K\left({\;\;b_1\;\;b_2\cdots\cdots b_2\;\;b_1\;\; \atop A,\,a_1,\,a_2\cdots a_2,\,a_1,\,A + x}\right)}{K\left({\;\;b_2\cdots\cdots b_2\;\;b_1\;\; \atop a_1,\,a_2\cdots a_2,\,a_1,\,A + x}\right)},$$

whence it can be shown that

$$x^2 K\left({\;b_2\cdots\cdots b_2\; \atop a_1,\,a_2\cdots a_2,\,a_1}\right) = K\left({\;\;b_1\;\;b_2\cdots\cdots b_2\;\;b_1\;\; \atop A,\,a_1,\,a_2\cdots a_2,\,a_1,\,A}\right)$$

and thus we have the theorem—

$$A + \frac{b_1}{\underset{*}{a_1} +}\,\frac{b_2}{a_2 +}\,\frac{b_3}{a_3 +}\cdots + \frac{b_3}{a_2 +}\,\frac{b_2}{a_1 +}\,\frac{b_1}{\underset{*}{2A} +}\cdots$$

$$= \sqrt{\frac{K\left({\;\;b_1\;\;b_2\cdots\cdots b_2\;\;b_1\;\; \atop A,\,a_1,\,a_2\cdots a_2,\,a_1,\,A}\right)}{K\left({\;b_2\cdots\cdots b_2\; \atop a_1,\,a_2\cdots a_2,\,a_1}\right)}} \quad . \quad . \quad \text{(XIV.)}$$

17. From (XIV.) it is easy to deduce a series of identities expressed in continuants, viz.,

$$\frac{K\left({\;\;b_1\;\;b_2\cdots b_2\;\;b_1\;\; \atop A,\,a_1,\,a_2\ldots a_2,\,a_1,\,A}\right)}{K\left({\;b_2\cdots b_2\; \atop a_1,\,a_2\ldots a_2,\,a_1}\right)} = \frac{K\left({\;\;b_1\;\;b_2\cdots b_2\;\;b_1\;\;b_1\;\;b_2\cdots b_2\;\;b_1\;\; \atop A,\,a_1,\,a_2\ldots a_2,\,a_1,\,2A,\,a_1,\,a_2\ldots a_2,\,a_1,\,A}\right)}{K\left({\;b_2\cdots b_2\;\;b_1\;\;b_1\;\;b_2\cdots b_2\; \atop a_1,\,a_2\ldots a_2,\,a_1,\,2A,\,a_1,\,a_2\ldots a_2,\,a_1}\right)}, \text{ \&c. (XV.)}$$

18. With the help of (XIV.) we can also establish an important proposition in reference to the well-known subject of the expression of the square root of an integer as a continued fraction with unit-numerators. The proposition is:—The general expression for every integer whose square root when expressed as a continued fraction with unit numerators has $q_1, q_2, \ldots q_2, q_1$ for the symmetric portion of its cycle of partial denominators is

$$\left\{\tfrac{1}{2}K(q_1, q_2, \ldots q_2, q_1)\,m - (-1)^l\,\tfrac{1}{2}K(q_1, q_2, \ldots q_2)\,K(q_2 \ldots q_2)\right\}^2$$
$$+ K(q_1 \ldots q_2)\,m - (-1)^l\,K(q_2 \ldots q_1)^2 \quad . \quad \text{(XVI.)},$$

l being the number of elements in the cycle.

This is established by taking the general expression for *every* such number, fractional as well as integral, viz.,

$$\frac{\mathrm{K}(\mathrm{A},\, q_1, q_2 \ldots q_2, q_1, \mathrm{A})}{\mathrm{K}(q_1, q_2 \ldots q_2, q_1)} \quad . \quad . \qquad (a),$$

and proceeding to determine what form for A is necessary and sufficient to make this expression integral. The form found is

$$\tfrac{1}{2}\mathrm{K}(q_1 \ldots q_1)m - (-1)^s \tfrac{1}{2}\mathrm{K}(q_1 \ldots q_2)\mathrm{K}(q_2 \ldots q_2),$$

and substituting this for A in (a), we arrive at the expression (XVI.) after some reduction.

19. Further, no integer can be found whose square root when expressed as a continued fraction with unit-numerators has $q_1, q_2 \ldots q_2, q_1$ for the symmetric portion of its cycle of partial denominators, unless either $\mathrm{K}(q_1 \ldots q_2)$ or $\mathrm{K}(q_2 \ldots q_2)$ be even. This is deducible from the preceding.

20. Many interesting results may also be arrived at in reference to the possibility of expressing in more ways than one by a continued fraction the square root of any number.

All that is requisite in order to find as an equivalent for any quadratic surd, $\sqrt{13}$ say, a periodic continued fraction with a period of any given number of elements, say 5, is the solution in integers of an indeterminate equation of the form

$$\frac{\mathrm{K}\begin{pmatrix} & b_1 & b_2 & b_3 & b_2 & b_1 \\ \mathrm{A}, & a_1, & a_2, & a_2, & a_1, & \mathrm{A} \end{pmatrix}}{\mathrm{K}\begin{pmatrix} & b_2 & b_3 & b_2 \\ a_1, & a_2, & a_2, & a_1 \end{pmatrix}} = 13.$$

21. This leads to the consideration of the various identical forms of periodic continued fractions, and on this subject much may be learned. As an instance, we may show how a continued fraction with unit-numerators, such as is found in the usual way as the equivalent of a quadratic surd, may always be reduced to a periodic continued fraction with only three elements in its period. The identity is

$$\mathrm{A} + \frac{1}{\underset{*}{a}+}\,\frac{1}{b+}\ldots\frac{1}{+\,b+}\,\frac{1}{a+}\,\frac{1}{\underset{*}{2\mathrm{A}}+}\ldots$$

$$= \mathrm{A} + \frac{\mathrm{K}(b\,c \ldots c\,b)}{\mathrm{K}(a\,b \underset{*}{\ldots} c\,b)+}\,\frac{(-1)^{s-1}}{\mathrm{K}(a\,b \ldots c\,b)+}\,\frac{\mathrm{K}(b\,c \ldots c\,b)}{\underset{*}{2\,\mathrm{A}}}+\ldots$$

where l is the number of elements in the period of the first fraction. This we may prove by deducing from the expression which is given by (XIV.) for the square of the right hand member, the expression also given by (XIV.) for the square of the left hand member.

Similarly, we may show that

$$A + \frac{1}{\underset{*}{a}} + \frac{1}{b} + \frac{1}{c} + \frac{1}{b} + \frac{1}{a} + \frac{1}{\underset{*}{2A}} + \ldots$$

$$= A + \frac{bc+2}{\underset{*}{abc+2a+c}} + \frac{1}{b} + \frac{1}{abc+2a+c} + \frac{bc+2}{\underset{*}{2A}} + \ldots$$

and many other such identities.

22. Lastly, it is easily demonstrated that the condition that any periodic continued fraction

$$A + \frac{a_1}{\underset{*}{b_1}} + \frac{a_2}{b_2} + \ldots + \frac{a_{n-1}}{b_{n-1}} + \frac{a_n}{\underset{*}{b_n}} + \ldots$$

may represent a quadratic surd is

$$K\begin{pmatrix} a_1 \, \ldots\ldots \, a_{n-1} \\ A, b_1 \ldots b_{n-2}, b_{n-1} \end{pmatrix} = K\begin{pmatrix} a_2 \, \ldots\ldots\ldots \, a_n \\ b_1, b_2 \ldots b_{n-1}, b_n - A \end{pmatrix},$$

and that this can be satisfied in other ways than by choosing the elements so that the diagonals of the one continuant when read forward may be the same as those of the other when read backward.

3. Remarks upon the Footprints of the Dinornis in the Sand Rock at Poverty Bay, New Zealand, and upon its recent extinction. By T. H. Cockburn-Hood, F.G.S.

Impressions of the tracks of large birds from this locality have lately been objects of attraction to visitors to the museum at Wellington, New Zealand. To these Dr Hector, F.R.S., has affixed a label, stating that they are from the "Sea shore sand" at Poverty bay, a harbour on the east coast of the north island. "Sand rock" would have been a preferable term, as to most observers the description is calculated to convey the idea that these footprints are but of yesterday's date. Indeed, were it not probable that the moa was

extinct in the northern island for a considerable time before it was exterminated on the opposite side of Cook's Straits (which is a matter still quite open to doubt), they might be merely the tracks of individuals, contemporary with that, the egg of which was found in the grave of the Hurunui chieftain, placed there to serve him as provision on his way to happier hunting grounds, and would thus lose much of the interest which appertains to them as very ancient memorials.

The present specimens were obtained by the writer on a late visit to the district of Poverty Bay.

The slabs were cut out of a bed of rock, crossing a small affluent which falls into the Turanganui river, near its mouth, and the footprints, first observed by the ferryman, and pointed out to Archdeacon Williams, are now washed by every tide. The deposit can be traced across the estuary to a point under the high land, on the northern shore of the bay, where similar impressions are to be seen.

It has been suggested that this bed is but a portion left of the ancient plateau composed of strata known to local geologists as the Hawke's Bay series, but no such antiquity can be assigned to it, having been formed from the detritus of the cliffs (which rival in whiteness the chalk walls of the English channel) swept into this spot by a current which eddied round under the precipitous coast, at a time when the shallow bay extended further inland, but when otherwise the configuration of the land was much the same as it is now.

From the number of the footprints crossing and recrossing each other, and the proximity of those of individuals, it seems that these birds were in the habit of resorting to the sea-shore to feed upon the small fish and mollusks left by the receding tide, as the Rheas of South America do at the present day.

The strata among which the impressions occur appear to be the result partly of the accumulation of blown sand, partly of subaqueous deposit during a period of gradual submergence.

At the mouth of the Hutt River, and along the shore of Wellington harbour, during the earthquakes of 1855, the land rose nine feet, and a corresponding depression took place of the valley, it is stated, in which the town of Blenheim is situated on the southern shore of Cook's Straits.

At one time this ornithichnite bed, now washed by every tide, was (as it is still beyond its influence) covered by many feet of the delta alluvium. The river Waipaoa, which formed these extensive plains of rich soil, averaging twenty to twenty-five feet in depth, now very rarely overflows its banks. Only once, in the memory of the oldest native, has it done so to any extent, and this was since the settlement of Europeans, on which occasion there was a deposit left of half an inch, in some few spots of an inch of silt; although in bygone times, under different cosmical influences, it probably discharged a much greater volume of water into the bay, at a point opposite the island on the northern shore, and left after every fresh a larger amount of soil than it does now on these rare occasions, a vast time must have elapsed since it left the first layer of mud over the sandstone bed.

Dr. Hochstetter, the accomplished naturalist who accompanied the Austrian expedition of 1859, remarks, "These gigantic birds belong to an era prior to the human race, to a Post-Tertiary period; and it is a remarkably incomprehensible fact of the creation, that whilst at the very same period in the old world, elephants, rhinoceroses, hippopotami—in South America, gigantic sloths and armadillos—in Australia, gigantic kangaroos, wombats, and dasyures were living,—the colossal forms of life were represented in New Zealand by gigantic birds." But whilst these gigantic birds have a higher antiquity than even the megatherium, the diprotodon, or zygomaturus, and other strange quadrupedal forms of life, which have long passed away, or left only puny representatives, like the æpiornis of Madagascar, which maintained its ground down to a late period in that great island, and against men, too, singularly, of an allied race to the Maorie, the moa has the credit of having held its own down to the present century, through all the great changes of scene and climate which have taken place since its ancestors stalked over the plains of the southern portion of a great land,—the backbone of which, and little more, remains,—perhaps with large lacertians for its companions, long after the giant marsupials, the contemporaries of its congener * on the Australian savannahs, had disappeared.

* The interesting discovery there of a large fossil bird has lately been made known by the distinguished geologist the Rev. W. B. Clarke, who first made

The evidences of the late existence of the moa are to be seen. It is not possible that the tender skull and small bones could have been preserved in the situations in which they have been found for any great lapse of time. Exposed to the fierce summer sun, and the severe winter frosts on the upper Otago plains, the bones of a bullock soon decay, but upon these downs nearly perfect skeletons of moas have been found amongst the high fern, with a heap of the so-called moa stones beside them, evidently undisturbed since the birds died upon the spot. The feathers in the museum at Wellington are some of those preserved by the chiefs in the carved boxes which most persons of distinction possessed for the purpose of keeping such prized ornaments. These, and the egg with the well-developed bones of the embryo chick, of which a photograph is here presented,—the extremely interesting relic, the cervical vertebræ of a moa, to which the skin, partially covered with feathers, is still attached by the shrivelled muscles and integuments, found in a cave in Otago formed by an overhanging cliff of mica schist,—are amongst the objects in that collection affording proofs almost incontrovertible, to say nothing of the traditions of the mode of hunting the grand quarry,* preserved in Maorie song and story.

The remains of these gigantic birds are common throughout both islands. No Maorie, upon being shown any of the principal bones, will hesitate in referring them at once to the moa. If credence is denied to their traditions, we are obliged to come to the conclusion that the different tribes possessed persons endowed with the acumen of a Cuvier or an Owen, who explained from

public the marvellous auriferous and general mineral wealth of that continent, and by his indefatigable researches has added so much to our knowledge of its strange denizens in the past, as well as at the present time. Professor Owen has named this bird dromornis, considering it to have been more allied to the emeu than the moa or apterix tribe.

* The paper was accompanied by two photographs. Of these, one was that of the skeleton of one of the largest specimens hitherto obtained. It is placed in the museum at Christ Church, New Zealand, beside that of a tall man. It was one of a great number dug up by Mr Moore at Glenmark in Canterbury province, in a piece of swampy ground, now transformed into a fine garden, which had been one of those places into which the bones of different individuals were washed from the hills around during freshes, and into which the moas rushed when driven by the fires kindled by the natives for the purpose of driving their game, dray loads of bones being here collected.

their knowledge of comparative anatomy that these huge remains appertained to birds. If the theory of the extinction of the dinornis before the arrival of the Maories be accepted, a very great age must be granted to these singularly well-preserved bones; for, from some of the traditions of those people, we are led to the conclusion that the date of their forefathers' landing in this country is much more remote than generally supposed.

It may be that, as well as possessing a knowledge of comparative anatomy, the Maorie fathers were also acute geologists; but it is much more probable that the poetical story of the quarrel of the three brother gods of the volcanos of Rua-pehu, Tonjoriro, and Taranaki, and the flight of the latter down to the plain which now bears his name, tearing up, as he fled, the deep gorge of the Whanjarioa river, the taking of the remarkable truncated cone of Ranjitolo* from the lake on the north shore of Auckland harbour, and other similar stories, have reference to memories of those great disturbances, when the almost matchless cone of Mount-Egmont was thrown up on the Taranaki shore, and the geyser circled lake of Taupo was formed, where the third great crater of the group formerly stood upon "that huge flat cone,"—the sterile pumice-stone plateau of Taupo,—events which took place at a period when the stepping-stones from New Zealand to the old home of the Maorie were probably not so far apart as they are to-day, as far back, it may be, as the time when the skeletons of men of this most ancient type, now from time to time exhumed from their graves deep in the solid limestone rock, covered with the ashes and scoriæ of long quiescent craters, lay bleaching upon the coral strand of Oahu.

* This volcano has evidently been quiet for a long period, but its name, "bloody heavens," denotes that it has not always been so, since the Maories first sailed up Hauraki Gulf.

Monday, 19th January 1874.

Principal Sir ALEX. GRANT, Vice-President, in the Chair.

The following Communications were read:—

1. Supplementary Notice of the Fossil Trees of Craigleith Quarry. By Sir R. Christison, Bart., Hon. Vice-President, R.S.E., &c.

This notice supplements that of 5th May last, which has been published in the Abstracts of the Proceedings of the Society.

Seven fossils, all apparently belonging to the Pine tribe, and either to the same species, or to two closely allied to one another, have been uncovered since 1826 in the sandstone of Craigleith Quarry. Six are stems of great trees; and one is a longitudinally split section of a large branch, or possibly of another stem. Portions of all seven have been traced as still in existence, and have been subjected more or less to examination. Of one, the greatest of all, about 36 continuous feet, from 12 to 14 feet in girth, have been removed in large fragments to the British Museum, and will be pieced and erected there. Another, found in 1830, is now partly in the Botanic Garden, and will be supplemented by other portions at present in the Museum of Science and Art, so as to make a nearly perfect fossil stem 30 feet in length. A third, nearly 9 feet in girth, has been sliced and polished, to show its structure on the great scale, and will be exhibited in the British Museum, the Edinburgh Museum, and the Edinburgh Botanic Garden.

The composition of all these great fossils is substantially the same. The great mass of each consists of carbonate of lime, carbonate of magnesia, carbonate of protoxide of iron, and free carbon, the proportions varying in different parts of the same fossil. The iron-carbonate and charcoal vary most in their amount. The charcoal, which is left after the action of diluted acids, sometimes without any other insoluble residuum, seems to form three per cent. of the mass, unless when collected, as it often is, in

cavities. This charcoal contains only about 3¼ per cent. of incombustible ash.

The surface of the fossils is covered with a shining coat of very bituminous caking coal, which on the principal part of the stem varied from only a 20th to a 10th in thickness, but at the lower end of that now at the British Museum, increased to half an inch, and at last to two inches and a half. This coaly covering contains only 4, 3, 2, and sometimes only 1·1 per cent. of mineral matter; which is not the same as the fossilising matter of the included wood, but is chiefly siliceous in nature, being at least insoluble in acids. The crust is not altered bark, for bark could not fail to undergo, in part at least, fossilisation by the material which has fossilised the wood. Moreover, the coaly crust is found round fragments and on broken points where bark could never have existed.

The rock of the quarry is a very pure quartzy sandstone, hard, tough, and quite free from earthy carbonates or iron. But for some feet around the fossils, and also here and there throughout the quarry, where there is no fossil near, the rock has quite a different appearance, has a higher density, is more sharp-edged, much tougher, and harder to pulverise, and becomes yellow under exposure to the air. These changes are owing to the siliceous particles of the sandstone being bound together by carbonate of lime, carbonate of magnesia, and carbonate of protoxide of iron, forming together from 10 to 38 per cent. of the rock, and bearing much the same relation in proportion to each other as in the mineral material of the fossils,—consequently derived from the same fluid which fossilised them.

Thus the interesting fact is presented of these great trees and the rock in which they are imbedded having been both similarly mineralised, so to speak, by the same fossilising fluid, while there is between them a thin uniform coating of bituminous coal, which has refused admission to any of the fossilising agents. After rejecting various theories to account for this exemption, the only one which stands the test of facts is, that a part of the process of fossilisation consists in a slow process, analogous in its results to the destructive distillation of wood, the result of which is charcoal left behind, and bitumen gradually forced outwards, and collected on the exterior surface.

The charcoal which remains in the stems renders their minute internal structure singularly distinct when a thin transparent slice is placed before the microscope. Longitudinal woody bundles, transverse medullary rays, crowded cells of the longitudinal fibres cut crosswise, are all seen most characteristically; and in one specimen two inches in breadth the boundaries and whole structure of five annual layers of wood are displayed characteristically, even to the naked eye. On the polished surface of one of the great stems, too, the eye can easily trace many annual rings for long distances.

2. On a Method of Demonstrating the Relations of the Convolutions of the Brain to the Surface of the Head. By Professor Turner.

The outer surface of the skull does not correspond in shape to the outside of the brain. If it had corresponded there would have been no difficulty in determining the form of the brain from an inspection of the form of the head.

The shape of the brain does correspond to the wall of the cranial cavity. This wall is formed by the inner table of the cranial bones, which table, though separated from the brain itself by the cerebral membranes, is moulded upon the exterior of the organ.

The difference between the form of the inner table of the skull and that of the outside of the cranium is owing to the superaddition of the diplöe and of the outer table, which superadded parts modify the shape of the outer surface of the skull.

The diplöe varies somewhat in thickness in different bones, or in different parts of the same bone, and even at different periods of life, and these variations necessarily cause the outer table to be removed to a greater distance from the inner table in some parts of the cranial wall than in others.

The outer table is modified in shape by ridges and processes for the attachment of muscles; *e.g.*, temporal ridge, curved lines of occiput, occipital protuberance, mastoid process; but in certain localities, as the superciliary ridges, glabella and mastoid processes, more especially in the male skull, it is still further modified by the hollowing out of the diplöe into the frontal and mastoid air cells

or sinuses, and the elevation of the corresponding part of the outer table.

These difficulties in the way of estimating the exact shape of the exterior of the brain, from an inspection of the outside of the head, were pointed out and discussed at the time when the phrenological systems of Gall and Spurzheim were advocated in this city by George Combe and his disciples.

But at that period an additional and even more important difficulty stood in the way of determining the exact relations of the outside of the brain to the outside of the skull, for the external configuration of the brain itself was not properly understood.

Spurzheim had undoubtedly recognised that, in general form and direction, the convolutions of the human brain are "remarkably regular." Thus he says—"The transverse convolutions of the superior, lateral, and middle parts of the hemispheres are never found running in any other direction—never longitudinally, for example. Those that lie longitudinally again, as they do under the squamous suture, behind the temporal bone and on either side of the olfactory nerve, are never met with disposed transversely."* His contemporaries Reil, Rolando, Foville, and Huschke had also directed attention to the constancy of individual convolutions. It was not, however, until the publication in 1854 of Gratiolet's great work on the cerebral convolutions† that the surface of the cerebrum was so mapped out that definite descriptive names were applied, not only to the several lobes, but to the individual convolutions composing them, and the constancy of their position and relations to each other precisely determined. The study of Gratiolet's work, and the adoption by so many anatomists of the greater number of his descriptive terms, have tended materially to advance our knowledge of the convolutions, and to make them more definite objects of physiological and pathological research. A need has therefore arisen for localising the position of the cerebral lobes and convolutions on the surface of the skull and head, and a method, or methods, of readily doing so is to be desiderated. In selecting names for four of the five lobes into which he subdivided each cerebral hemisphere, Gratiolet employed terms which expressed

* The Anatomy of the Brain, translated by Willis, p. 111. London, 1826.

† Mémoires sur les Plis Cerebraux. Paris, 1854.

the relations he believed to exist between these lobes and the vault of the skull, *e.g.* frontal, parietal, occipital, and temporo-sphenoidal lobes. In an essay published in 1861,* M. Broca pointed out that the frontal bone was not equal in extent to the frontal lobe, but that the fissure of Rolando was invariably some distance behind the coronal suture. In eleven males examined the minimum distance of the upper end of this fissure was 40 mm., the maximum 63 mm. from the suture. He further stated that a constant relation existed between the lambdoidal suture and the fissure which separates the parietal from the occipital lobe. He never found the suture more than 15 mm. from the fissure, rarely more than 5 mm. M. Broca's method of determining these relations was by drilling holes in the skull, inserting wooden pegs into the brain, and then, after removing the skull cap, ascertaining the part of the surface of the hemisphere into which the pegs had penetrated. Almost similar results were obtained by Professor Bischoff by pursuing the same mode of examination.†

This plan of drilling holes through the skull, and inserting pegs through them into the brain, is one which may be conveniently employed when the object is merely to obtain an idea of the extent of the lobes of the cerebrum in relation to the surface of the head, as only a few holes require to be bored to effect this object. But as the operation of drilling a number of holes through the cranial bones demands the expenditure of much time and labour, it is not very convenient if it is desired to fix the position of the individual convolutions. It occurred to me, therefore, that some other method might be resorted to to effect this object.

As a preliminary measure, I sub-divided the surface of the skull into regions: a præ-coronal or frontal, the region of the frontal bone; a parietal, sub-divided into antero- and postero-parietal by a vertical line drawn upwards from the squamous suture through the parietal eminence to the sagittal suture; a post-lambdoidal or occipital, between the lambdoidal suture and the superior curved line of the occiput; a squamosal and a sphenoid, corresponding to the squamous temporal and to the great wing of the sphenoid. The line of the temporal ridge sub-divides the antero- and postero-

* Sur le Siége de la Faculté du Langage articulé. Paris, 1861.
† Die Grosshirnwindungen des Menschen. Munich, 1868.

parietal into a supero- and infero-anterior and supero- and infero-posterior parietal regions, and marks off also an infero-frontal area on the frontal bone. The frontal bone may be still further subdivided into a supero-and mid-frontal region by a longitudinal line drawn back from the upper border of the orbit through the frontal eminence to the coronal suture.

With a fine saw I then cut out, one after another, the pieces of bone along the lines which constituted the boundaries of these different regions, and examined with care the particular convolution, or group of convolutions, which lay immediately subjacent to the portion of bone removed. In this manner I was able to localise in the specimens examined the relations of the convolutions to the surface of the skull and head. As I have already detailed the results of my examinations in the "Journal of Anatomy and Physiology," November 1873, I need not repeat them here; but it may not be out of place to point out that the lobes of the brain by no means precisely correspond to the areas of the cranial bones, after which four of them are named. The frontal lobe is not only covered over by the frontal bone, but extends backwards for a considerable distance under cover of the parietal bone. If we accept, as I have elsewhere described,* the fissure of Rolando as the posterior limit of this lobe, then the larger part of the antero-parietal region corresponds with the frontal lobe, for not only does it contain the origins of the superior, middle, and inferior frontal gyri, but also the ascending frontal convolution. But even if we were to regard the ascending frontal gyrus, and not the fissure of Rolando, as bounding the frontal lobe posteriorly, the frontal lobe would still not be wholly localised under cover of the frontal bone, for the superior, middle and inferior frontal gyri all arise from the ascending frontal gyrus, behind the line of the coronal suture.

The occipital lobe also is not limited to the region covered by the squamous part of the occipital bone, but slightly overlapping the lambdoidal suture, extends forwards for a short distance into the back part of the upper postero-parietal area, and through the superior annectent gyrus reaches the parieto-occipital fissure.

* Edinburgh Medical Journal, June 1866, and separate publication, "The Convolutions of the Human Cerebrum topographically considered."

The superior temporo-sphenoidal gyrus, though for the most part situated under cover of the squamous-temporal and great wing of the sphenoid, yet ascends into both the lower antero- and lower postero-parietal areas.

The area covered by the parietal bone, so far then from being conterminous with the parietal lobe of the cerebrum, is trenched on anteriorly, posteriorly and inferiorly by three of the other lobes of the brain. The convolutions of the parietal lobe itself are especially grouped round the parietal eminence, and in the interval between that structure and the sagittal suture.

The Insula or central lobe does not come to the surface, but lies deep in the Sylvian fissure, and is concealed by the convolutions which form the margin of that fissure anteriorly. It lies opposite the upper part of the great wing of the sphenoid and its line of articulation with the antero-inferior angle of the parietal and the squamous part of the temporal.

3. On some Peculiarities in the Embryogeny of *Tropæolum speciosum*, Endl. & Poepp., and *T. peregrinum*, L. By Professor Alexander Dickson.

4. Notes on Mr Sang's Communication of 7th April 1873 on a Singular Property possessed by the Fluid enclosed in Crystal Cavities in Iceland Spar. (1.) By Professor Tait; (2.) By Professor Swan.

(1.) Professor Tait.

The very beautiful experiment of Mr Sang, communicated to the Society on the 7th April, 1873, suggested to me, as soon as I heard him read his description of it, an explanation which was confirmed by a subsequent examination of his specimens. Some remarks made to me by members of the Council of the Society, three days afterwards, led me to write, and deposit (under seal, as Mr Sang had announced that he was still prosecuting his inquiry) with the Secretary the following hastily written docu-

ment, which has been since that time in his possession, and is now printed *verbatim* :—

"*April 10th*, 1873.

"Carbonic Acid—partly liquid, partly gaseous—fills the cavity.

"Distillation, when one end is heated ever so slightly above the other, the circumstances being of almost unexampled favourability for such an effect. Hence the *apparent* motion of the bubble. IT IS NOT THE SAME BUBBLE AS IT MOVES.

"General problem suggested by this, and easily solved by the dynamical theory of heat.

"Find distribution of LEAST ENTROPY of contents of a vessel where the temperature is a given function of the position in space, and the contents are one or more substances (say, for simplicity, not chemically acting on one another) in two or more different states (as to latent heat, &c.)

"This is *more* (MUCH MORE) than the whole affair.

P. G. TAIT."

A day or two afterwards I tried the experiment on a large scale, with the assistance of my laboratory students, and at once succeeded in showing to them, and to several of my colleagues, Mr Sang's results in quill tubes of three or four inches in length, containing sulphurous acid partly in the liquid and partly in the gaseous state.

The present communication, like that of Professor Swan which follows it, is now made to the Society at the request of Mr Sang himself.

(2.) Professor Swan.

The following note is a narrative of experiments made by me nine months ago, on the 5th and 6th May 1873, on the motions observed in the cavities of Iceland spar by Mr Sang, with an explanation of the manner in which I believe these singular movements to be caused by heat. Being unwilling to interfere with Mr Sang's investigations then in progress, I did not at the time seek to publish my note, but forwarded it in a sealed envelope to the secretary of the Society, in whose custody it has since remained. It is now communicated to the Society in accordance with Mr Sang's wishes, and is printed without alteration or addition.

On Certain Motions observed by Mr Sang in Cavities of Iceland Spar. By Professor W. Swan, LL.D.

I have received from my friend, Mr Edward Sang, a crystal of Iceland Spar with a letter dated 1st May, in which he writes as follows:—

"In the accompanying little bit of Iceland spar you will find a number of microscopic cavities of various shapes, in which you may perceive a small bubble of vapour, which serves to show the movement of the enclosed fluid."

The glass slide carrying the crystal being placed horizontally on the stage of a microscope, if "you bring a piece of metal, say a coin, gradually until its edge come almost into the field of view, you will see all the bubbles take the (apparently) opposite sides of their cavities: that is to say, the metal repels the fluid. On inclining the microscope the bubbles take the tops" of their cavities, and "you will find that the repulsion exceeds gravity in intensity. I have only found this repulsion with metals: oxides and sulphurets have no action, and each metal has its own specific repulsion. Silver is more active than lead, and, if I mistake not, also than gold. Mercury has little or no effect."

To-day (5th May) I had no difficulty in verifying Mr Sang's result as to the motion of the vapour bubbles when a coin touching the Iceland spar was brought near the fluid cavities, but the experiments I was thereafter induced to make lead to conclusions in some respects differing from those which he has obtained.

Having placed his specimen of Iceland spar on the stage of an excellent Ross's microscope belonging to the United College, and using a one-inch object glass, I saw distinctly the motion of the vapour bubbles, when a florin piece *taken out of my pocket* was brought up, touching the surface of the spar so to come into the field of view, and nearly to cover the fluid cavity observed. The apparent effect was the *attraction* of the vapour bubble, which always ran to the side of the cavity nearest to the edge of the coin. I could distinctly mark the tendency which the bubble exhibited to run in a direction normal to the edge of the piece of metal.

Before having tried any experiments, and while meditating on Mr Sang's letter, I could not help concluding that most probably

heat would prove to be the agent which caused the curious motions which he had observed. I therefore placed the coin outside the window to be cooled in the east wind, and the rain which was falling plentifully. I found that the cold coin caused no sensible motion of the bubbles. I next heated the coin in a spirit lamp flame as hot as I could conveniently handle it. Its energy in moving the bubbles was now so greatly increased, that in some trials rapid motions were observed while the coin was still *out* of the field of view.

Seeing that the bubbles thus moved towards the heated side of their cavities, I concluded that they ought to be repelled from a side which was cooled: and to try if such were the case, I cooled a florin piece in a freezing mixture of nitre, sal ammoniac, and water, to a temperature below 0° C. I had now the satisfaction to find that the cold coin, resting on the spar and brought up towards a cavity, sent the bubble away to the remote side of the cavity, just as the hot coin had brought it to the near side.

It is clear, then, that the phenomenon is not due to a repulsion of the liquid in the cavity by the piece of metal, but is a consequence of the passage of a heat current through the liquid, the bubble always moving in a direction opposite to that in which heat is flowing.

I found that metals possess no specific property in causing these motions. The bubbles moved on the approach of a silver coin or a copper wire. But similar motions were readily obtained when, instead of these, were substituted a heated rod of glass, a slender thin test tube containing hot water, or a piece of shellac moulded into a pencil shape, and still hot from the flame employed to soften it. All these substances—glass, water in the thin tube, and shell-lac—when cooled in the freezing mixture, repelled, or seemed to repel, the bubble, just as when heated they had attracted, or seemed to attract it. The temperatures in these experiments were not accurately observed, but they must have been as follows:—Coins taken from my pocket would be hotter than the air of the room, which was 10° C. or 50° F. The coins being of silver, an excellent conductor of heat, would, when held in the hand, be hotter than the spar lying on the microscope stage in air at 10° C. The coins taken from the flame were as hot at first as could well be handled, and

therefore hotter than the spar. The freezing mixture at the conclusion of the experiment had still a temperature of – 5° C. or 23° F.

I found that the direction of motion of the bubbles was the same whether a heated copper wire was held above or below the Iceland spar, with the slide resting horizontally on the stage of the microscope. I fully verified Mr Sang's statement regarding the motion of the bubbles when the microscope is inclined. Placing its tube horizontally, so that the face of the stage and the glass slide were vertical, the bubbles, of course, all rose to the tops of their cavities. A hot copper wire or silver coin touching the surface of the spar at a point on a lower level than that of one of the cavities, instantly drew the bubble down to the bottom. The motion in a vertical plane with a tolerably hot wire seemed almost as brisk as it had been in a horizontal direction, so as to indicate that the effect of hydrostatic pressure, due to gravity, on the minute bubble was trifling as compared with the action set up by the heat current.

Considering the enormous dilatability by heat of liquids, which, under ordinary conditions of temperature and pressure, are permanent gases, I at first thought the motions of the bubbles might be due to currents caused by unequal heating of the liquid on opposite sides of the cavities. The heat flowing from a piece of metal brought near a cavity would cause dilatation of the liquid on the nearer side. A current would then evidently flow along the upper surface of the cavity away from the heated metal, and carry a bubble resting at the top in that direction. But this is precisely the reverse of the motion actually observed; so admitting, as we can scarcely doubt, the setting up of a current due to unequal heating, there must be some other and more energetic action at work, causing a real or apparent motion against any such current; and this I take to be rapid evaporation and condensation of the liquid on opposite sides of the bubble. Suppose A B to be a bubble floating in a cavity through which a heat current passes in the direction A B. A state of equilibrium of the bubble is then evidently impossible. Liquid will evaporate from the hotter side A of the bubble, and vapour will condense into liquid on the colder side B. The liquid surface A will then, by continual loss, travel in the direction B A, and the surface B will by continual gain follow A in the same direction; so

that there will be an apparent motion of the bubble towards the hotter end of the cavity,—an *apparent* motion only, for in reality it is not one and the same mass of vapour which is travelling through the liquid. Any such identical bubble has only a momentary existence. It is continually being changed into a new bubble in a

new position by the accretion of vapour on the side A, and by the restoration of vapour to the liquid state on the side B; and the change of place of the existing bubble is in the direction from B to A, or in a direction opposite to that of the heat current. Such a motion, it is scarcely necessary to remark, agrees with that which is actually observed.

The action thus set up in a vapour bubble is precisely that which takes place in Wollaston's cryophorus, where vapour, rapidly generated at the hotter, is recondensed into liquid and frozen at the colder end of the apparatus. Suppose a cryophorus to consist simply of a cylindrical tube placed vertically and cooled by a freezing mixture at its upper end. The cavity occupied by vapour will then suffer a continual displacement downwards; for the surface of the water, which is its lower boundary, is being depressed through loss by evaporation, while the glass at the top, which was at first its upper boundary, is becoming coated with ice of constantly increasing thickness. The downward displacement of the cavity of such a cryophorus may serve to illustrate that of a bubble in a liquid heated from below. But it may seem that any movement thus produced would be far too slow to displace a bubble downwards, which was rising freely through a liquid. In considering such an objection to the proposed explanation of the motions of bubbles in the cavities of crystals, when these motions take place vertically, or otherwise than in a horizontal direction, it is to be borne in mind,

—first, that the upward motion through a liquid of a *microscopic* bubble must necessarily be very slow, even although under a high magnifying power it may seem otherwise; and next, that in space containing only vapour of a liquid in contact with the liquid itself, evaporation and recondensation may proceed with excessive rapidity. The action of Wollaston's cryophorus, to which reference has just been made, and Dalton's experiments on vapours, made by passing liquids up into a Torricellian vacuum, alike exhibit the facility with which vapours form and recondense in spaces void of gases which are permanent at the existing temperature. Add to these considerations the information derived from the experiments of Cagniard de la Tour, Faraday, and Andrews, as to the enormous celerity with which substances pass from the liquid to the gaseous, or from the gaseous to the liquid condition, when near their critical temperatures, which for different substances range probably between the very remote limits 773° and −166° Fahrenheit, and the explanation which I have ventured to propose of the motion of a vapour bubble in a liquid conveying a heat current becomes sufficiently feasible.

I have to-day been at some pains to verify the result obtained yesterday, namely, that a piece of metal at the same temperature as the Iceland spar has no power to move the globules of vapour in the fluid cavities. Placing a shilling on the microscope stage beside the crystal, I left it for about ten minutes. Then holding it in forceps to avoid heating it by the hand, I moved it up into the field touching the spar, and so as almost to cover a fluid cavity from view. No motion of the bubble ensued. But, on putting my finger on the top of the shilling, by-and-by the bubble began to move, and slowly but steadily crossed the cavity towards the shilling. The same experiment was repeated with a bit of sheet lead about an inch square and 0·08 inch thick, with precisely the same result. I do not find lead notably less active than silver; but the experiments made were necessarily too hasty and imperfect to settle the point as to whether any difference exists. The relative thermal conductivities of silver and lead being in air as 100 to 8·5, according to Wiedemann and Franz's experiments, we might expect, when heat was conducted from the hand into the crystal through a piece of metal, that silver would produce more energetic effects than lead. May the effects be due, in part at least, to radiant heat, the

liquid in the cavities being possibly less diathermanous than the Iceland spar, and absorbing the heat transmitted to it by radiation through the crystal?

In order to try if the motion of a vapour bubble could be exhibited on a larger scale, I made use of a hermetically sealed tube containing liquefied sulphurous acid (sulphur dioxide) which I had some time ago prepared to show the high dilatability by heat of that liquid. When the tube was placed horizontally the void space, like the bubble of a spirit level, was about 15 inches long; and I found that its extremity moved towards the point where a piece of heated brass was applied to the tube. I then nearly filled a tube with ether made from methylated alcohol; and after heating the top, so as to vaporise the ether and expel the air, I hermetically sealed the tube. Placing the tube horizontally, the vapour bubble is about 0·3 of an inch long; and when a finger is put on the tube about 0·25 of an inch from the bubble, in a little while the bubble moves towards the finger with a rapidly accelerated motion, and places itself in a position of stable equilibrum under the finger, about which it slightly oscillates even after the finger is removed from the tube. A piece of metal too hot to be touched acts still more energetically.

I have thought it proper to note that the ether I used had been made from methylated alcohol, because in exhibiting as a lecture experiment Dalton's method of measuring vapour tensions, I have found that ether made from methylated alcohol seems to show a higher vapour tension than that of ether as determined by Regnault. This is probably due to the presence of some other substance more volatile than common or diethyl ether, possibly to a portion of dimethyl ether whose boiling-point is so low as $-21°$ C.

United College, St Andrews,
7th May 1878.

5. Preliminary Note on the sense of Rotation and the Function of the Semicircular Canals of the Internal Ear. By Professor A. Crum-Brown.

As far as I am aware, the sense of rotation has not hitherto been recognised either by physiologists or by psychologists as a distinct sense, but a little consideration and a few experiments seem to me to be enough to show that it really is so. By means of this sense we are able to determine—*a*, the axis about which rotation of the head takes place; *b*, the direction of the rotation; and *c*, its rate.

In ordinary circumstances we do not wholly depend upon this sense for such information. Sight, hearing, touch, and the muscular sense assist us in determining the direction and amount of our motions of rotation, as well as of those of translation; but if we purposely deprive ourselves of such aids we find that we can still determine with considerable accuracy the axis, the direction, and the rate of rotation. The experiments that I have made with the view of determining this point were conducted as follows: a stool was placed on the centre of a table capable of rotating smoothly about a vertical axis; upon this the experimenter sat, his eyes being closed and bandaged; an assistant then turned the table as smoothly as possible through an angle of the sense and extent of which the experimenter had not been informed. It was found that, with moderate speed, and when not more than two or three complete turns were made at once, the experimenter could form a tolerably accurate judgment of the angle through which he had been turned. By placing the head in various positions, it was possible to make the vertical axis coincide with any straight line in the head. It was found that the accuracy of the sense was not the same for each position of the axis in the head, and further, that the minimum perceptible angular rate of rotation varied also with the position of the axis.

The sense of rotation is, like other senses, subject to illusions, rotation being perceived where none takes place. Vertigo or giddiness is a phenomenon of this kind.

When, in the experiments just mentioned, rotation at a uniform

angular rate is kept up for some time, the rate appears to the experimenter to be gradually diminishing; if the rotation be then stopped, he experiences the sensation of rotation about the same axis in the opposite direction. If the position of the head be changed after the prolonged rotation has been made, the position of the axis of the apparent rotation is changed, remaining always parallel to a line in the head which was parallel to the axis of the real rotation. The readiness with which this *complementary apparent rotation* is produced is not the same for each axis. In such experiments, as long as the eyes are shut, and the axis of rotation kept vertical, a sensation of giddiness is not experienced. That sensation appears to be caused by the discordance between the testimony of the sense of sight and that of the sense of rotation.

It is obvious that this sense must have a peripheral organ physically constituted so as to be affected by rotation, and that it must be such as to receive different impressions when the axis, direction, or rate of rotation is changed. These impressions must be transferred to the ends of afferent nerves, and by these nerves conducted to a central organ.

The semicircular canals of the internal ear are eminently fitted by their form and arrangement to act as the peripheral organ of this sense. I shall consider first the action of one semicircular canal, and for simplicity suppose that there is only one. Starting from rest, let us suppose rotation of the head to take place about an axis at right angles to the plane of the canal. The bony canal, being part of the skull, of course shares in this rotation, but the perilymph lags behind, and thus the membranous canal, which floats in the perilymph, does not immediately follow the motion of the bony canal, but, as the membranous canal is continuous at both ends with the utricle, the relative motion of the bony and the membranous canal must produce a pulling or stretching of the forward end of the membranous canal. If this is the end at which the ampulla is situated, such stretching will necessarily move the terminal nervous organs in the ampulla, and may reasonably be expected to stimulate the nerves. This stimulus will no doubt be greater the stronger the pull, *i.e.*, the more rapid the rotation. We should thus with one semicircular canal have the means of perceiving, and of estimating the rate of, rotation in

one direction about *one* axis. But we have *six* semicircular canals, three in one ear and three in the other, and these are arranged in pairs—the two exterior being nearly in the same plane, and the superior in one ear being nearly parallel to the posterior in the other. We have thus a system of three rectangular axes, each axis having two semicircular canals at right angles to it,—one influenced by rotation in one direction about the axis, the other by rotation in the opposite direction. Any rotation whatever of the head can be resolved into three rotations, one about each of the said three rectangular axes, and will thus in general affect three ampullæ. If the ampullæ affected are known, and the amount of pull at each is known, the axis about which rotation takes place and the rate of the rotation can be deduced.*

I am at present engaged in making measurements and experiments in reference to this inquiry, and hope before long to lay a more complete account of the various phenomena before the Society.

* When rotation has continued for some time, friction of the periosteum of the bony canals against the perilymph, and fluid friction in the perilymph, gives to the perilymph, and, of course, also to the membranous canal, the same rotation as the bony canal has; the perception of rotation will thus cease. If we now stop the rotation of the head the bony canal stops, but the perilymph and the membranous canal move on, and a pull takes place at the opposite ends of the semicircular canals, causing a perception of rotation round the same axis in the opposite direction.

The members of the three pairs of semicircular canals are not always accurately parallel to each other, and in some animals the three axes are not accurately at right angles, so that in the most general case we have two systems of co-ordinates, not necessarily rectangular, which we may call x, y, z, and ξ, η, ζ—each of these six axes having an organ capable of being influenced by rotation about the axis *in one direction*. But in all cases, as far as I know, these six axes and the corresponding organs are so placed that a different set of impressions will be produced by each form of rotation, that is, by each combination of axis, direction, and rate.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VIII. 1873–74. No. 88.

NINETY-FIRST SESSION.

Monday, 2d February 1874.

Professor Sir WILLIAM THOMSON, President, in the Chair.

The following Obituary Notices of Deceased Fellows of the Society were read:—

1. Biographical Notice of J. S. Mill. By Professor Fraser.

John Stuart Mill was born in London on the 20th of May 1806, and died at Avignon on the 8th of May 1873. He was of Scotch descent. He was connected with Edinburgh not only as having been an honorary member of this Society, but because his father, James Mill, the historian of British India, and author of the "Analysis of the Human Mind," received his academical education here. His grandfather was a small farmer, at Northwater Bridge, in the county of Angus, of whom I find nothing more recorded. The father, by his extraordinary intellectual promise when a boy, drew the attention of Sir John Stuart, then member for Kincardineshire, by whom he was sent to the University of Edinburgh, at the expense of a fund, established by Lady Jane Stuart and some other ladies, for educating young men for the Church of Scotland. Towards the end of last century, James Mill attended the classes in Arts and Divinity. He was a pupil of Dalziel, the Professor of Greek, whose prelections he attended, I believe, for three sessions, and his philosophical powers were called forth by Dugald Stewart's

lectures in Moral Philosophy. I do not know by what Presbytery he was licensed to preach, but I have heard Sir David Brewster say that he had listened to one of his sermons. When a student at the University it seems that he was given to reading books of a sceptical tendency in religion. He soon found the ministry uncongenial to him, having satisfied himself that he could not believe the doctrines of any Christian Church.

About the year 1800 James Mill removed to London, where for nearly twenty years he made his living by his pen. He was a man of singular force of character and subtlety of intellect—a stern Scotch Stoic or Cynic, with an Epicurean creed. He married soon after he settled in the metropolis, with only the precarious income of a literary adventurer. The eldest son of his large family was John Stuart Mill. He was born about the time the "History of India" was begun. In the twelve following years the extraordinary energy of the father was chiefly given to this great work, and to the instruction of his eldest son.

That eldest son has himself described in his "Autobiography" some of the original influences by which his own mind and character were formed. The stern paternal schoolmaster was one of the most important. The story of young Mill's early instruction is as extraordinary as any in the records of English training. Books in Greek, Latin, and English; in history, logic, and analytical psychology, were among the means—the end being the production of as perfect a reasoning machine as could be produced out of the boy. What is commonly included in the higher education began with him in childhood. He was introduced to Greek when he was three years of age. Before he was eight he had read many Greek books, including the Theætetus of Plato. He had also read a great deal of history, including Hume and Gibbon, and had discussed what he had read with his father, in their rural walks about Newington Green, where the Mills were living from 1810 to the end of 1813. In the winter of 1813 they moved into a house, rented from their friend Jeremy Bentham, in Queen Square, Westminster. About the time this change was made young Mill began to learn Latin. Before he was twelve he had read most of the Latin and Greek poets, historians, and orators, much of the Rhetoric of Aristotle, and a great deal of ancient history. At twelve his philosophical

education began. He studied logic in the Organon, in Latin treatises of scholastic logic, and in Hobbes. His later experience made him set great value on this early familiarity with Aristotelian logic. The first intellectual operation in which he arrived at proficiency was dissecting bad arguments, and finding in what the fallacy lay. Ricardo and a course of political economy followed; also much study of Plato. The high expectations his stern and exacting preceptor had of him at this time appear in a letter from James Mill to Jeremy Bentham in 1812.

In May 1820 he was sent to France. His father had in the year before been appointed one of the Assistants of the Examiner of Correspondence in the India House. Abroad the boy lived in the family of Sir Samuel Bentham, a brother of Jeremy. He was introduced to M. Say, the political economist, and other French *savans* in Paris. This was the beginning of the intimate sympathy with the literary and political society of France, which was always characteristic of John Mill.

In July 1821 he returned to England. He resumed his old studies, with the addition of some new ones. He read Condillac "as much for warning as example." In the winter of 1821-22, he studied jurisprudence under John Austin, and also in the writings of his father's friend, Jeremy Bentham. His whole previous education had been in a certain sense a course of Benthamism, for he had been always taught to apply Bentham's standard of "the greatest happiness." He lived much in Bentham's society, and often accompanied him and his father in their walks together, at Newington Green and afterwards in Westminster, besides making long summer visits to him at Ford Abbey, in Devonshire. Before he was fifteen, his studies were carried into analytic psychology, still under his father's direction. He read Locke, Berkeley, Helvetius, Hartley, Hume, Reid, Stewart, and Brown on "Cause and Effect." The elder Mill about this time began to write his "Analysis of the Human Mind," which was published seven years later, in 1829, and the son was allowed to read the manuscript, portion by portion, as it advanced.

This training, while it produced an astonishing precocity of logical intelligence, was not equally favourable to physical vigour, and practical skill or sagacity. Mr Mill tells us that as he had

no boy companions, and the animal need of physical action was satisfied by walking, his amusements, which were mostly solitary, were in general of a quiet if not bookish turn, and gave little stimulus to any other kind of mental acting than that which was already called forth by his studies. He consequently remained long, and in a less degree always remained, inexpert in everything requiring manual dexterity, and his mind as well as his hands did its work lamely, when it was applied to the practical details which are the chief interest of life to the majority of men. He was constantly meriting reproof by inattention, inobservance, and general slackness of mind in matters of daily life.

Beauchamp's "Analysis of the Influence of Natural Religion on the Temporal Happiness of Mankind" (papers of Bentham edited by Grote) was read by young Mill. This was an examination not of the truth, but of the usefulness of religion, and suited his mental condition well. His father had educated him from the first without any religious belief. The elder Mill, "finding no halting-place in Deism, had yielded to the conviction that nothing whatever can be known concerning the origin of things." He impressed upon his son from the first that the manner in which the universe came into existence was a matter on which nothing was or could be discovered; that the question, "Who made me?" cannot be answered, because we can have no experience from which to answer it; and that any answer only throws the difficulty a step further back, since the question immediately presents itself, "Who made God?" He assumed it to be impossible that a world so full of evil could be the production of a cause combining infinite power with perfect goodness. John Mill was thus, he says himself, "one of the very few examples in this country of one who has not thrown off religious belief, but who has never had any." He looked upon the modern exactly as he did upon the ancient religion, as something which in no way concerned him. If a philosopher has to comprehend what exists, it was unfortunate for Mr Mill, and unfavourable to the comprehensiveness of his philosophy, that he should have thus been trained to overlook Christianity, the greatest fact in European life.

Other than home influences now began to have play. In May 1829 his professional occupation was determined. He became a

subordinate in the India House under his father, who was resolved not to leave him to the uncertainty of the adventurous literary life. Steady official duties in Leadenhall Street occupied him in the thirty-five following years, at the end of which the East India Company was extinguished as a governing power. But his duties there always allowed him to have time enough for study.

He was now introduced gradually to a wider companionship. In the winter of 1822–23, he had formed the plan of a little society, composed of young men acknowledging Utility as the standard in ethical and political thought. At his suggestion it was called the Utilitarian Society. It was the first time that any one had taken the title of Utilitarian; but the term soon made its way into the English language. John Austin, William Ellis, John Arthur Roebuck, George Grote, and others, appear among his friends and associates.

He began about this time to show himself in print. His first published writings were two letters, which appeared in the end of 1822, in the "Traveller" newspaper, in defence of some opinion of Ricardo and his father in political economy. Early in the following year he published some letters in the "Morning Chronicle," in favour of complete freedom of religious discussion, in connection with the trial of Richard Carlile for blasphemy. During 1823 several of his writings appeared in the "Traveller" and "Morning Chronicle."

In April 1824 the "Westminster Review" was started, under the auspices of Jeremy Bentham, with John Bowring as editor. From that time till July 1828 Mr Mill was its most frequent contributor. He wrote thirteen articles in these years. One is especially worthy of note,—a review of Whately's "Logic," which appeared in January 1828, which it is interesting to compare with the modification and extension of the science proposed fifteen years afterwards in his own System. In 1827, at Bentham's request, his name was given to the world as editor of that philosopher's greatest treatise, the "Rationale of Evidence," the preface to which was written by Mr Mill: his previous publications were anonymous. This work, and the annotations, occupied much of his time for about a year. The connection of the subject with the form which logic afterwards took in his own hands is manifest.

In these years various influences helped to show that he had a nature too deep and human to be satisfied with the hard Benthamite creed in which he was trained. For some years after 1828 he wrote little, and nothing regularly, for publication. He congratulates himself on this. If he had gone on writing, it would have disturbed, he thinks, an important transformation in his opinions and character which was taking place about this time. For years his one object in life had been to be a reformer of society. He was now awakened from this as from a dream. All his happiness was to have been found in the steady pursuit of this end: the end, he found, had ceased to charm him, and he seemed to himself to have nothing left to live for. He was weighed down by melancholy. Part of the explanation probably was that his nerves were exhausted by an early life too purely intellectual. His condition so far reminds one of the account which David Hume gives of himself in the very curious letter to a physician, written at a corresponding period of life, and preserved among the papers in the possession of this Society, published by Mr Burton in his "Life of David Hume." It is interesting to compare Hume's story, in that letter, and Mr Mill's in his "Autobiography." The health of both seems to have been broken for the time by a too ardent application to abstract studies. The truth, however, was that Mill had discovered in some degree the narrowness of the theory of life on which his early training had been based. It had left him nothing worth living for. Mill, like Hume, gradually recovered, but with a more marked change in his mental tone and opinions afterwards than one finds in Hume. His early Utilitarianism was modified. While still convinced that happiness was the chief end of human life, he now, with doubtful consistency, thought that this was to be attained by not making it the direct end; and that those only are happy who have their minds fixed on some object other than their own happiness—the philanthropic improvement of mankind, for instance. He found, too, that the emotions needed to be cultivated as well as the intellect. He began to feel the importance of poetry and art, especially music, as instruments of human culture. He was always very fond of music, and a scientific proficient.

The reading of Wordsworth for the first time, in the autumn of

1828, was an important event in Mr Mill's life. Beauty in nature had a power over him then that was a foundation for his taking pleasure in Wordsworth's poetry. He became a Wordsworthian, and contended on this side against Roebuck in a Debating Society. His sympathies were carrying him more and more away from Benthamism, and towards a deeper and truer philosophy of life. He was brought into friendly companionship with Frederick Maurice, and John Sterling, and other admirers of Coleridge. He became one of Coleridge's occasional visitors at Highgate, to whom I have heard that he was introduced by Sir Henry Taylor. After 1829 he withdrew from the Debating Society, and pursued his studies and meditations in private, endeavouring thus to adjust the relation of his new ideas and sympathies to his old opinions. Indeed, after this he seems to have lost his early fondness for Societies for discussion: a few years ago he declined to connect himself with the lately-founded Metaphysical Society of London, having the opinion that valuable results in subjects of abstract philosophy are best attained in solitary dialectic, or with a single interlocutor.

In the Society from which he withdrew, logical questions had been often discussed. About 1830 he began to put on paper thoughts on the theory of logic, and especially on the relations of induction to syllogism. Thus his own system of logic began to take shape. In political philosophy, too, he began to see that the truth was something more complex and many-sided than his early instruction had presupposed. This tendency was encouraged by a sympathetic study of the writings of the St Simonian school in France, and of the early works of Auguste Comte. Thomas Carlyle, too, had an effect upon him. He felt himself at an increasing distance from his father's whole tone of thought and feeling.

The year 1830, above all, was the commencement of what he considered the most valuable friendship of his life—that of Mrs Taylor, who, twenty years afterwards, became his wife, and whose influence over him, for good or evil, marked the whole remainder of his course.

About 1832 and the two or three following years of political excitement, he published writings in the "Examiner" and other

newspapers, and in the "Monthly Repository," which were more according to his matured judgment than his previous periodical essays.

His father died in June 1836. This seems to have freed him from some restraints and reticences. His friend Sir William Molesworth, a political and metaphysical thinker, had proposed to found a new Review, provided Mr Mill would agree to conduct it. In this way he was editor of the "London"—latterly the "London and Westminster—Review" in the years between 1835 and 1840. This Review was the organ which he then used for the spread of his opinions. It enabled him to express in print the results of his altered modes of thought, and to separate himself in a marked manner from the narrower Benthamism of his early writings. He resigned the editorship in 1840, after which he usually preferred for his essays the wider circulation of the "Edinburgh Review."

The first use Mr Mill made of the leisure gained by freedom from the cares of a brilliant editorship was to resume his "Logic." The preparation of this historically important treatise had occupied him at intervals for twelve years. In 1841 it was ready for the press, but circumstances delayed the publication till the spring of 1843. He now appeared for the first time as the author of a book, and of his greatest book—"A System of Logic, Ratiocinative and Inductive, being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation." It is the most elaborate treatise in the English language on the logical procedure in Induction. Since the publication of the "Novum Organum" and the "Essay on Human Understanding," no such comprehensive attempt in logical theory and the principles of the formation of knowledge had been made by an Englishman. Mr Mill had not forgotten his early studies in Aristotelian logic, which, in his correlation of induction and syllogism, he tried to assimilate with the methods of modern science. If we do not accept the result as satisfactory, we may at any rate allow that it has usefully called attention to the one-sidedness of merely formal logic. If he fails to show that all inference is ultimately from observed particulars to unobserved particulars, without any need for general notions, he has at least helped to prove the fruitlessness of merely verbal

syllogising, and to show the part which facts have in all our actual reasonings. It is as a logician probably that Mr Mill will be longest remembered in the history of English and European thought, and as having connected the revived logical studies of this country with the spirit and procedure of modern experimental science.

The same decade which gave birth to Mr Mill's "Logic" saw the first publication of the other great treatise of his life—next in importance to his "Logic." In 1848 his "Principles of Political Economy, with some of their Applications to Social Philosophy," were given to the world. Through this book he became to the nineteenth century in some degree what Adam Smith had been to the eighteenth by his "Wealth of Nations." It had been heralded in 1844 by "Five Essays on some Unsettled Questions in Economic Science." The "Political Economy" showed a return in some particulars from his previous extreme of reaction against his early Benthamism, along with a disposition to sceptical criticism of many of the presuppositions of the older school of political economists. His ideas of ultimate social improvement were becoming more revolutionary. His view of private property was becoming modified, and especially of the rights of individuals to land. Co-operation and Socialism began to take the place of Competition and Democracy in his thoughts.

The "System of Logic" and the "Principles of Political Economy" are the two books round one or the other of which almost all that Mr Mill has ever written may be said to circulate. The one describes his view of the intellectual means; the other is connected with the aim or end of the whole labour of his manhood. The logical employment of intellect for the improvement of society was in brief his life. Eight editions of the "Logic" have now been published; the "Political Economy," after passing through seven editions, was issued in a cheap form in 1865.

The ten years which followed the publication of the "Political Economy" formed a long pause in Mr Mill's course as an author. He was married to Mrs Taylor in April 1851, her former husband having died two years before. They lived in extreme seclusion for some years, withdrawn even from the society of his intimate friends, and under influences which tended again to confine his

sympathies. The silence was broken only by an occasional article in the "Edinburgh Review," or by replies to criticisms on one or other of his two great books.

Changes now occurred. In 1856 he was made Examiner of Indian Correspondence, and thus placed at the head of the office in the India House, in which he had served for thirty-three years. In the following year the Government of India was transferred from the Company to the Crown; after an unavailing remonstrance, drafted by Mr Mill, in the name of the Court of Directors, which was pronounced by Lord Grey the ablest State paper he had ever read. He afterwards declined an invitation by the present Lord Derby, then Indian Secretary, to form one of the newly-constituted Board of Indian Council.

Mr Mill had arranged to spend the winter of 1858-59—the first after his retirement from office—in the south of Europe. The death of his wife at Avignon, on their journey, frustrated his plans and hopes. The profound effect of this event upon his feelings is expressed in the most touching sentences he ever wrote, and to which there are few parallels in literature. It induced him to settle as near as possible to the place where she was buried. It thus became his habit to spend a great part of each year in his cottage at Avignon.

He soon reappeared as an author. His essay on "Liberty" was published in 1859. It had been planned and written as a short paper in 1854. It was in mounting the steps of the Capitol in the following year that the thought suggested itself of converting it into a volume. The essay is a vindication of the importance to society, and for the discovery of truth, of giving men full freedom to expand themselves in opposite and even conflicting directions, limited only by the prevention of injury to others. This little volume may be supposed to have had no inconsiderable effect in promoting that toleration for the free expression of opinion, even regarding beliefs longest reverenced, which, compared with the past, is a remarkable characteristic of this generation in Great Britain.

In the same year Mr Mill republished, in a collected form, in two volumes, under the title of "Dissertations and Discussions," articles formerly contributed to the "London," "London and West-

minster," and "Edinburgh" Reviews, as well as to other periodicals: a third volume followed in 1867. A pamphlet of "Thoughts on Parliamentary Reform" was also produced in 1859. In 1861 he published "Considerations on Representative Government."

In 1862 the essay on "Utilitarianism" appeared. It contains his latest view of ethical theory, and of the new criterion of morality which it was one great endeavour of his life to make known.

Mr Mill's principal contribution to analytical psychology and metaphysics was made in 1865. It took the form of an "Examination of Sir William Hamilton's Philosophy;" a large and elaborate volume, equal in scope and comprehensiveness to his greatest works. The "Examination" is a sort of philosophical supplement to his "Logic," in which many of the principles here argued had been silently assumed. Its tendency is to promote an explanation, through circumstances and association, of beliefs and feelings, which are apparently necessary and universal; in opposition to those who treat them as ultimate elements of human nature, and even as absolute or ontological necessities of reason. By Mr Mill this, like other questions, was not regarded as a mere matter of abstract speculation. Like his illustrious predecessor Locke, he thought he saw, in a prevailing tendency to consider some principles to be independent of the verification of experience, one of the most powerful obstructions to the efforts of the social reformer; and, like his predecessors on the same path, it may be thought that his theory makes science speculatively impossible for man. If rationality in nature is the basis of science, knowledge must presuppose reason in nature as the condition of its own existence; and then all ordinary inductive verification proceeds on the assumption of beliefs which do not admit themselves of being verified by observation.

This remarkable essay in metaphysics was followed by an essay in which he offers his final estimate of "Auguste Comte and Positivism."

After this productive literary period, Mr Mill was withdrawn for three years from his studious seclusion at Avignon. At the general election in 1865 he was chosen member for Westminster, and he appeared in the House of Commons when Parliament met in February 1866. In that and the two following sessions he was an

active and deeply interested member of the House of Commons—sessions of Parliament which passed the second Reform Bill. He spoke occasionally, and was heard with respect and curiosity, as the representative of large philosophical principles and a sort of philanthropic socialism. The advocacy of women's suffrage is that perhaps with which his Parliamentary name is most associated. In these years in England, he lived at Blackheath.

One result of the general election in November 1868 was to send Mr Mill back to his old pursuits, and to seclusion at Avignon. The Parliamentary episode had not indeed entirely interrupted his studies. In 1866 he read through Plato, as a preparation for a review of Grote. A fervid pamphlet in the same year, on "England and Ireland," urged a radical reform in the land system of the sister island. In 1867 he delivered an elaborate address on the Higher Education to the students of the University of St Andrews, who had chosen him as their Rector. He was also employed about a new edition of his father's "Analysis of the Human Mind," in conjunction with Mr Grote, Professor Bain, and our townsman Dr Findlater, which was published in 1869.

The years which followed Mr Mill's short Parliamentary career were mostly spent at Avignon, where he continued his life of literary labour. His essay on the "Subjection of Women" appeared in 1869, and this, with his efforts in Parliament, helped to make the education, and the political and social condition of the sex one of the questions of the day. His last published writing in philosophy of which I am aware was a review, in November 1871, of the Clarendon Press edition of Berkeley's works. He had always been a great admirer of Berkeley. In this essay he expresses the opinion that "of all who from the earliest times have applied the powers of their minds to metaphysical inquiries, Berkeley was the one of greatest philosophical genius; though among these are included Plato, Hobbes, Locke, Hartley, and Hume, as well as Des Cartes, Spinoza, Leibnitz, and Kant." But it was the negative and analytic side of Berkeley that he admired; he had no appreciation of the constructive part of his doctrine, on which Berkeley himself lays most stress.

In March of last year, Mr Mill visited London, and lived for six weeks in a suite of rooms he had taken in Victoria Street,

Westminster. He spoke at a meeting on the land question, in support of his opinion with regard to "the unearned increment in the value of land." He had previously published "Chapters and Speeches on the Irish Land Question," followed by a "Programme of the Land Tenure Reform Association." During these weeks in London he mixed much in society. The writer of this Notice spent part of Mr Mill's last day in England with him in his rooms in Westminster, when he seemed full of physical and intellectual vigour, and indulged in youthful recollections of his father and of Bentham. Next day, the 18th of April, he returned to Avignon. On Saturday the 3d of May, he made a long botanising excursion in that neighbourhood. Botanical research had been an enthusiasm of his life, and his original collection of herbaria is, I believe, of great value. He caught a chill on his way home. It issued in a severe form of erysipelas, of which he died on the morning of the following Thursday. He was buried the day after beside his wife. The Protestant pastor, the physician, and his domestic servant, formed the small company of mourners who saw him laid in his grave.

Mr Mill's appearances in public in his later years, aided by the art of the photographer, have made his earnest, thoughtful face, with its sensitive, nervous action, familiar to many. A refined, delicate organism, and wiry form, suggested the moderately good health which, notwithstanding extraordinary intellectual labour he enjoyed through life. He was fond of walking; allured by his love of botany and his passion for rural nature. He was a great reader of all sorts of current and periodical literature. His conversation, like his books, was remarkable for its abundance of logically digested information, judicially deliberate, distinct, and everywhere vivified by the presence of active intelligence. He showed little or no appreciation of humour, but both his spoken and written words revealed a subdued and grave emotional fervour, especially for the propagation of opinions in which he believed, and the promotion of social changes which he supposed to be advantageous.

Probably no contemporary has modified more than Mr Mill the tone and manner of thinking of the fairly-educated community in Great Britain. The time is hardly come, however, for a satisfac-

tory estimate of what he has done, what he has failed to do, and what his influence in the future is likely to be. The habit of thinking characteristic of this generation is too much affected by his logical methods, and pervaded by his spirit, to admit of a perfectly just estimate.

That he has been in a great degree the representative English thinker of his generation will be generally allowed; for we already see enough to recognise in him the leader in this age of that school of British philosophy, which, in the seventeenth century, was represented by Hobbes and Locke, and in last century by Hartley and Hume. If he wanted the rugged masculine vigour and originality of Hobbes, he had more ardent sympathies and a more indulgent candour. Locke undoubtedly far excelled him in massive common sense and in practical knowledge of human nature, and was more complete as a man; but he was hardly superior as a subtle analytical psychologist, or equal as a lucid expositor. If Mr Mill wanted Hume's grace, humour, gaiety of temper, and insight, in the expression of a philosophy of life in a large degree common to them both, he had a moral earnestness and intensity of sentiment which one does not find in Hume. Mill was eminently a logician rather than a metaphysician or a speculative moralist; his conception of life was limited in its scope and aim. He methodised the experience of an age devoted to the physical sciences, and tending towards materialism. He was not a speculative philosopher, who sought to comprehend the universe: he was a reformer who wanted to make society better, by improving its relations to its circumstances on this planet. He accordingly explained to his countrymen their own scientific habits of research, in which inductive methods and presuppositions are employed with extraordinary vigour and success, for the improvement of circumstances and of the external arrangements of society. As a metaphysician, he always tried to keep speculation within the limits of positive science, and to dissolve by analysis, as hurtful prejudices, the faith or thought which does not admit of ordinary inductive verification,—thus, it may be alleged, overlooking in man, and withdrawing from human life, some of their best and noblest possessions.

Yet in some of their aspects Mr Mill's life and writings witness to a broader and deeper philosophy than he professed. His heart and his

sympathies outgrew the adverse influences of a sunless childhood. And his doctrines in metaphysics and ethics sometimes, I think, unconsciously recognise principles which break the logical symmetry of his professed Utilitarianism and philosophy of Custom and Association, producing, as in the case of Locke and others, an ambiguity in the exposition of his most important conclusions. As Sir James Mackintosh suggests of David Hume, it would indeed be a matter of wonder if his esteem for moral excellence should not at least have led him to envy those who are able to contemplate the perfection of excellence in the Supreme Reason that is accepted by them as the support of their lives, and the all-reconciling unity of existence.

2. Obituary Notes of the Rev. Dr Guthrie. By the Rev. Dr Lindsay Alexander.

Dr Thomas Guthrie was a native of Brechin, where he was born on the 12th of July 1803. His father, David Guthrie, was one of the principal merchants in that ancient city, and long occupied an influential position in it, being versant in all its affairs, and for several years holding the place of chief magistrate. Thomas was his sixth son. Having received a sound elementary education under different teachers in Brechin and the vicinity, Thomas was, at the early age of twelve, entered as a student in the University of Edinburgh; and there, for ten consecutive sessions, he continued prosecuting studies through the prescribed curriculum in arts and divinity, with the addition of certain branches of natural science, to which he spontaneously betook himself. In 1825 he received from the Presbytery of Brechin license as a preacher, and began forthwith to preach as occasion presented itself. Shortly after he was offered the presentation to an important charge, but as the offer was clogged with conditions which appeared to him to threaten his independence of thought and action he declined it; and no other professional opening appearing he went to Paris, where, for the best part of a year, he prosecuted medical studies at the Sorbonne, attending the lectures of Gay-Lussac, Thenard, and St Hilaire, and witnessing surgical operations by Dupuytren and Lisfranc at the hospitals. On his return home, being still dis-

appointed in his professional prospects, he purposed spending a year at one of the German universities, but from this he was turned aside in consequence of the death of his elder brother, who was a banker in Brechin, and who, dying somewhat suddenly, left his business in danger of being transferred to other hands, unless some one should be found to carry it on until such time as his son, then a boy, should be able to succeed him. In this emergency the only one of the family who was free to come to the help of the minor was his uncle Thomas, and he at once threw himself into the breach, and for two years conducted the business of the bank. On this he looked back with satisfaction as affording not the least valuable part of his training and education, as it brought him acquainted with the busy world, enlarged his knowledge of men and things, and gave him an aptitude for the management of affairs of which he found the advantage in after life. Whilst engaged in the business of the bank he did not intermit his studies or neglect opportunities of preaching when these were offered to him. He thus let it be known that he had no intention of abandoning his proper profession, and was only waiting till some suitable sphere was opened for him to enter upon the active discharge of its duties. Such a sphere was at length obtained by his being presented to the church and parish of Arbirlot, in Forfarshire, where he was ordained minister on the 13th of May 1830. Here he continued to labour with much assiduity and success for seven years, caring not only for the spiritual interests of his people, but bringing all the resources which previous culture and observation, as well as natural ability and good sense, had enabled him to accumulate, to bear upon the promotion of their temporal welfare. Here he laid the foundation of that eminence as a preacher which he afterwards attained, and here also he entered on that acquaintance with the condition, habits, wants, and perils of the poor, which in after years he turned to such excellent account in his philanthropic efforts. The fame of his power in the pulpit as a preacher, as well as of his administrative ability in his parochial cure, having reached the metropolis, where personally he was a stranger, he was in 1837 presented to the church and parish of Old Greyfriars, Edinburgh, as colleague with the late Rev. J. Sym. This charge he accepted, on the understanding that he would

exchange it for a single charge as soon as arrangements could be made for erecting a new parish in one of the more densely crowded and spiritually destitute parts of the city. This was accomplished when the new church was built in what used to be the West Bow, but where Victoria Street now stands; and on this Mr Guthrie entered as the minister of the new parish of St John's in 1840, determined, as far as in him lay, to work out the theory of the old parochial system in the centre of the city, and among a population many of whom were sunk in vice and degradation. Here he continued till the great secession from the Church of Scotland in 1843, when, having cast in his lot with the retiring party, of whose principles he cordially approved, and in whose proceedings he had taken an active share, he resigned his parochial charge and removed from the church of St John's, carrying with him his congregation. After some time, during which he preached in the Methodist Chapel, Nicolson Square, a new place of worship was erected not far from that which he had left, and to this, which came to be called Free St John's, he removed in 1844. In this church, where subsequently he had for his colleague the Rev. Dr Hanna, he continued to preach from Sunday to Sunday to audiences which crowded every corner, where room to sit or to stand could be found, for twenty years. During this period he was undoubtedly the most popular preacher in Scotland, perhaps in Britain. Persons of all ranks, and of every variety of culture, were found among his regular auditors; and illustrious strangers, statesmen, economists, and men of literature who visited the city, were often seen in the crowded pews. The care which he bestowed on the preparation of his discourses, the skill with which he arranged his topics, the vigour and perspicuity of his style, and, above all, the felicity of his illustrations and the truth and vividness of his descriptions, with the earnestness of his tone and the ease and naturalness of his delivery, combined to secure him this pre-eminence among the pulpit orators of his day.

But it was not only in the pulpit that, at this time, Mr Guthrie distinguished himself and drew to him popular esteem and homage. Even more, perhaps, as a philanthropist than as a preacher was his fame spread through the community. In him all good causes found an able and willing advocate; but it is chiefly with efforts

for the prevention of intemperance, and the rescue of destitute and degraded children, that his name is associated. Though not exactly the founder of ragged schools, he was the first to take a just estimate of their importance, the first to arouse the community in their favour, and the first to organise them formally and on an adequate scale; and to his powerful advocacy and persevering assiduity and care it is chiefly owing that these institutions are now so firmly established throughout the kingdom, where they have largely contributed to diminish pauperism, prevent crime, and add to the industrial strength of the nation. If his efforts for the suppression of intemperance have not met with the same success it is not because these were put forth with less zeal, perseverance, and self-denial on his part, but because the evil has grown to such a gigantic height as to render almost hopeless all attempts to remove or cure it. Nor, in referring to his labours for the benefit of others, should his great effort to raise money for the erection of comfortable residences for his brethren in the ministry be overlooked or mention of it omitted,—an effort to which, at a great amount of personal sacrifice, he devoted an entire year, traversing the country from end to end, visiting family after family, "from Cape Wrath to the Border, and from the German to the Atlantic Ocean," and bringing into the treasury of his Church, for the purpose he had in view, upwards of L.116,000. It was when appearing on the platform, as the advocate of such schemes of benevolence, that he came out in all his strength as an orator. On such occasions all his faculties had full play, and his mastery over his audience was complete—at one time guiding their judgments by reasoning and strong good sense, at another, bearing them along on the stream of impassioned declamation—now melting them to tears by some deep touch of pathos or some thrilling tale of sorrow or of suffering, and anon convulsing them with laughter by some rich stroke of humour, some amusing description, or some ludicrous anecdote. The only weapon of the orator which he did not use was sarcasm, for which his kindly nature had no taste.

In recognition of his abilities and valuable public services, the University of Edinburgh conferred on him, in 1849, the degree of D.D. In May 1862 he was raised to the Moderator's chair in the

twentieth General Assembly of the Free Church of Scotland,—a dignity which, in all probability, would have been conferred on him some years earlier had the state of his health permitted him to undertake the duties of the office.

Gifted with a vigorous constitution, Dr Guthrie had enjoyed good health, notwithstanding the excitement and toil attendant on the discharge of his official functions and his philanthropic efforts. But the continuous over-exertion to which he was exposed, espepecially in connection with the Manse scheme, began at length to tell upon him, and alarming symptoms, the prelude of that disease which ultimately carried him off, became apparent. By the advice of medical friends he was induced, though reluctantly, to retire from the public exercise of his ministry, and from all engagements that might have an exciting effect upon the system. This took place in 1864, when a valuable testimonial was presented to him, amounting to L.5000, contributed by friends and admirers in all parts of the kingdom. On his retirement from the pulpit, Dr Guthrie devoted himself chiefly to literary pursuits. He became editor of the "Sunday Magazine," and contributed largely to its pages. Whilst thus employed he found time to make repeated excursions to the continent; and of his contributions to the "Sunday Magazine" not the least striking and instructive is a series of papers containing graphic sketches of what he saw when abroad, with characteristic observations and reflections on the scenes and incidents he describes. Most of his papers in the magazine were subsequently collected and published separately. These, with some volumes of sermons and a few pamphlets, comprise Dr Guthrie's efforts as an author. His writings have been widely circulated in Great Britain, the colonies, and the United States, and have afforded instruction and delight to thousands who never saw his face or heard his voice.

After his retirement from the pulpit Dr Guthrie was enabled to continue his literary labours in the enjoyment of a considerable measure of vigour till towards the close of 1872, when his illness began to assume a more virulent form. In the beginning of the following year he went to St Leonards-on-the-Sea, to obtain the benefit of the milder climate of that locality; and there, on the 24th of February, he closed his mortal career. His remains were

brought to Edinburgh, and were interred in the Grange Cemetery. The funeral was attended by a very large company, including the magistrates and council of the city, ministers of nearly every denomination, both in the city and from different parts of the country, representatives of various public bodies, the directors and children of the Original Ragged School, as well as the personal friends and relations of the deceased. The procession extended for about three quarters of a mile, and moved through an immense crowd of people of all classes, assembled to show the last mark of respect to one than whom no citizen of Edinburgh was better known or more universally esteemed, as well for his private virtues and noble character as for his unwearied exertions for the benefit of others, especially for the relief of the destitute and the recovery of the fallen.

3. Obituary Notice of Mr R. W. Thomson. By Professor Fleeming Jenkin.

Mr R. W. Thomson, most widely known as the inventor of the road-steamer, died on the 8th of March 1873, in the fiftieth year of his age. By his death the community has lost a distinguished engineer, a remarkable thinker, and a highly original inventor.

Born in 1822, in Stonehaven, Mr Thomson furnishes one more example of the many Scotchmen who by sheer force of character, without any adventitious aid, have risen to be leaders in their profession and benefactors to their country. His father started on a small scale the only factory which even now Stonehaven possesses, and destined his eldest son (the subject of our memoir) to the pulpit, but the lad showed such dislike to classical studies that he was sent to Charleston, U.S., at the age of fourteen, to be educated as a merchant. Commerce proved as distasteful as the classics, and he returned at the age of sixteen to this country, where he began his self-education, aided materially by a weaver who chanced to be a mathematician.

Now, when scientific and technical education is almost thrust upon careless students, it is well to remember how this able and successful engineer acquired his knowledge, and to learn that energy in the pursuit of science is far more important than the

most elaborate machinery for its distribution. At this time Mr Thomson conceived the idea of the ribbon saw, afterwards worked out by other hands. The elliptic rotary steam-engine, to which he afterwards gave much time, was also then first conceived by him. He gained some experimental knowledge of chemistry and electricity, and his successful application of these sciences in after years proves the rare judgment with which he directed his studies. A short practical apprenticeship in workshops at Aberdeen and Dundee formed the next step in his education. He had great pleasure in telling how the foreman at the end of the first fortnight's work paid him more than he expected to receive, and when the apparent error was pointed out, told him that there was no mistake, "he was worth it." He was next employed by a cousin, Mr Lyon (the builder of the Dean Bridge), in connection with the blasting by which Dunbar Castle was blown down, and on this occasion conceived the happy idea of firing mines by electricity. Having brought his idea into a practical form, he went at the age of nineteen to London. Faraday, to whom the invention was shown, gave him hearty encouragement; and Sir William Cubitt was so much struck by the idea that he at once gave him an important charge in connection with the blasting operations then in progress near Dover. About this time he was engaged with a civil engineer in Glasgow, and subsequently passed into the employment of the Stephensons.

At the time of the railway mania, he was twenty-two years old, and began business on his own account, having a large staff, at ten guineas per diem, engaged in making plans and surveys for a line in the Eastern counties. He even achieved a triumph over Stephenson before a Parliamentary Committee, having refused to withdraw from competition at the instance of influential directors. The route he had chosen was ultimately adopted, although by other men, as the railway panic at the time stopped the undertaking.

Debarred by the result of the panic from prosecuting his profession as a business, Mr Thomson began again to invent, and devoted much time to the introduction of india-rubber tires, which he patented. The patent was not profitable, for the material was scarce and dear, and its manufacture ill understood; but he was fortunately rewarded at a later date by finding an important and

successful application for these tires in connection with his road-steamer. At this period of comparative leisure, he read much, and probably laid the foundation for that great cultivation and wide range of information which were so remarkable in the later years of his life.

When railway business revived, he did not seek to re-enter on the practice of this branch of his profession, which had no attractions for him, partaking as it does more of the nature of commerce than science. As a boy he nearly lost his place in the workshop by refusing to repeat some operation with which he was familiar, and as a man he never cared for the familiar routine which is most profitable. He sent in a creditable design for the great Exhibition of 1851, and a little invention of his, "the fountain pen," was sold in the building. In 1852 he went as agent for an engineering firm to Java, to erect some sugar machinery, and here he found a new field in which his powers could be worthily exerted. Although without capital, he was offered and he accepted a partnership in an important house shortly after his arrival. He then designed machinery for the manufacture of sugar so greatly superior to anything previously in use in the island, as to give a great impulse to the production of that commodity; and up to the time of his death he continued to supply the best machinery used in Java, where his honourable character commanded the unbounded confidence of the Dutch planters.

We owe perhaps the most universally useful of Mr Thomson's inventions to the refusal of the Dutch authorities to allow him to erect a waterside-crane, unless it could be removed every night, lest the natives should stumble over it. Mr Thomson hereupon designed the first portable steam-crane. He did not patent the idea, but Messrs Chaplin, who made the first small steam-crane for him, had, when he next re-visited England, two large factories engaged in the manufacture of these now indispensable appliances. The invention consisted mainly in employing the boiler as a counterpoise. In 1860 he re-visited Europe, to order a hydraulic dock consisting of a few types or classes of plates, each plate being interchangeable with every other plate of its class. He by this plan avoided the expense of double erection in England and abroad. The first dock thus made sunk when erected, in Mr

Thomson's absence, owing to the inexperience of the young engineer to whom it was intrusted. Fortunately two other docks from Mr Thomson's designs were in course of construction,—one for the French Government at Saigon, and the other for a company at Callao. These have been thoroughly successful.

In 1862 Mr Thomson retired from business in Java and settled in Edinburgh. He devoted much time and labour to perfecting the elliptic rotary engine, a clear and simple model of which may be seen in the Industrial Museum. His next invention, the Road Steamer, was the result of a direct practical want. An efficient traction engine was required for the transport of sugar-canes in Java, and none could be found capable of doing the work. Mr Thomson recurred to his old idea of india-rubber tires, and found in these a solution of the main difficulty in designing a traction engine. The tires are not fastened to the wheel, but adhere to it by friction. They form a broad pad or elephant's foot, by which the great weight of the engine is distributed over a large surface. The outer surface adapts itself to every peculiarity of the ground, and the inner surface forms, as it were, a constant endless platform on which the comparatively rigid engine works. The india-rubber does in a thoroughly practical manner what Boydell attempted to do by his impracticable endless railway. Both inventors wished to enable the steam-engine to work under constant conditions, but Mr Thomson's plan is strong, simple, and yielding, where Boydell's was weak, complex, and rigid. The perfect success of the plan is perhaps best attested by the numerous imitations which it has called forth, the object in most of these being to dispense with the expensive material india-rubber. The steel-protecting grooves for the tires are a later invention, and only a day or two before his death the inventor made an important improvement in their construction.

The zeal and energy of the true inventor in conquering difficulties and discouragement have often been told. Those who had the privilege of knowing Mr Thomson have seen this spectacle heightened in tragic interest by the grandeur of mind with which he contended against the terrible malady which has so much too soon closed his labours. If mental and moral qualities could be as simply described as mere mechanical inventions, more should

be said of the man, and less of the engineer. No written record can express the singular powers of Mr Thomson's mind and the charm of his character. The specialist in science, the professed chemist, the professed electrician, the professed geologist, the professed lawyer, all received suggestions from his fertile mind. The able and original paper on coal, read in this Society shortly before his death, affords an illustration of this sagacity of thought on subjects not specially his own. In art he had a cultivated taste, in narration and conversation he was unrivalled. All who conversed with him felt that they had never spoken so well themselves, and had seldom met with so sympathetic a listener. He had an untiring toleration for the failings of mankind, without abating for an instant in its application to himself the high standard which he shrank from applying to others. Even under terrible pain, his enjoyment of truth, of nature, of all that was noble, seemed not to flag. He never repined, but worked to the last hour, not with mere resignation, but with a noble contentment.

4. Obituary Notice of Archibald Smith. By Sir William Thomson.

[Abridged (by direction of the Author) from *Proc. R. S.*]

ARCHIBALD SMITH, only son of James Smith, of Jordanhill, Renfrewshire, was born on the 10th of August 1813, at Greenhead, Glasgow, in the house where his mother's father lived. His father had literary and scientific tastes with a strong practical turn, fostered no doubt by his education in the University of Glasgow, and his family connection with some of the chief founders of the great commercial community which has grown up by its side. In published works on various subjects he left enduring monuments of a long life of actively employed leisure. His discovery of different species of Arctic shells, in the course of several years' dredging from his yacht, and his inference of a previously existing colder climate in the part of the world now occupied by the British Islands, constituted a remarkable and important advancement of geological science. In his "Voyage and Shipwreck of St Paul," a masterly application of the principles of practical seamanship renders St Luke's narrative more thoroughly intelligible to us now

than it can have been to contemporary readers not aided by nautical knowledge. Later he published a "Dissertation on the Origin and Connection of the Gospels," and he was engaged in the collection of further materials for the elucidation of the same subject up to the time of his death, at the age of eighty-five. Archibald Smith's mother was also of a family distinguished for intellectual activity. Her paternal grandfather was Dr Andrew Wilson, Professor of Astronomy in the University of Glasgow, whose speculations on the constitution of the sun are now generally accepted, especially since the discovery of spectrum-analysis and its application to solar physics. Her uncle, Dr Patrick Wilson, who succeeded to his father's Chair in the University, was author of papers in the "Philosophical Transactions" on Meteorology and on Aberration.

Archibald Smith's earliest years were chiefly passed in the old castle of Roseneath. In 1818 and 1819 he was taken by his father and mother to travel on the continent of Europe. Much of his early education was given him by his father, who read Virgil with him when he was about nine years old. He also had lessons from the Roseneath parish schoolmaster, Mr Dodds, who was very proud of his young pupil. In Edinburgh, during the winters 1820–22, he went to a day school; and after that, living at home at Jordanhill, he attended the Grammar School of Glasgow for three years. As a boy he was extremely active, and fond of everything that demanded skill, strength, and daring. At Roseneath he was constantly in boats; and his favourite reading was anything about the sea, commencing, no doubt, with tales of adventurers and buccaneers, but going on to narratives of voyages of discovery, and to the best text-books of seamanship and navigation as he grew older. He had, of course, the ordinary ardent desire to become a sailor, incidental to boys of this island; but with him the passion remained through life, and largely influenced the scientific work by which he has conferred never-to-be-forgotten benefits on the marine service of the world, and made contributions to nautical science which have earned credit for England among maritime nations. He was early initiated into practical seamanship under his father's instructions in yacht sailing. He became an expert and bold pilot, exploring and marking passages and anchorages for

himself among the intricate channels and rocks of the West Highlands, when charts did not supply the requisite information. His most loved recreation from the labours of Lincoln's Inn was always a cruise in the West Highlands. In the last summer of his life, after a naturally strong constitution had broken down under the stress of mathematical work on ships' magnetism by night, following days of hard work in his legal profession, he regained something of his health and strength in sailing about with his boys in his yacht, between the beautiful coasts of the Firth of Clyde, but not enough, alas! to carry him through unfavourable influences of the winter that followed.

In 1826 he went to a school at Redland, near Bristol, for two years; and in 1828 he entered the University of Glasgow, where he not only began to show his remarkable capacity for mathematical science in the classes of Mathematics and Natural Philosophy, but also distinguished himself highly in classics and logic. Among his fellow-students were Norman Macleod and Archibald Campbell Tait, with both of whom he retained a friendship throughout life. After completing his fourth session in Glasgow, he joined in the summer of 1832 a Cambridge reading party, under Hopkins, at Barmouth in North Wales, and in the October following commenced residence in Trinity College, Cambridge.

While still an undergraduate he wrote and communicated to the Cambridge Philosophical Society a paper on Fresnel's wave-surface. The mathematical tact and power for which he afterwards became celebrated were shown to a remarkable degree in this his first published work.

In 1836 he took his degree as Senior Wrangler and first Smith's Prizeman, and in the same year he was elected to a Fellowship in Trinity College.

Shortly after taking his degree, he proposed to his friend Duncan Farquharson Gregory, of the celebrated Edinburgh mathematical family, then an undergraduate of Trinity College, the establishment of an English periodical for the publication of short papers on mathematical subjects. Gregory answered in a letter of date December 4th, 1836, cordially entering into the scheme, and undertaking the office of editor. The result was the "Cambridge Mathematical Journal," of which the first number appeared in

November 1837. It was carried on in numbers, appearing three times a year, under the editorship of Gregory, until his death, and has been continued under various editors, and with several changes of name, till the present time, when it is represented by the "Quarterly Journal of Mathematics" and the "Messenger of Mathematics." The original "Cambridge Mathematical Journal" of Smith and Gregory, containing as it did many admirable papers by Smith and Gregory themselves, and by other able contributors, early attracted to it, among whom were Greatheed, Donkin, Walton, Sylvester, Ellis, Cayley, Boole, inaugurated a most fruitful revival of mathematics in England, of which Herschel, Peacock, Babbage and Green, had been the prophets and precursors.

It is much to be regretted that neither Cambridge nor the university of his native city could offer a position to Smith, enabling him to make the mathematical and physical science for which he felt so strong an inclination, and for which he had so great capacity, the professional work of his life. Two years after taking his degree, he commenced reading law in London, but his inclination was still for science. Relinquishing reluctantly a Trinity Lectureship offered to him by Whewell in 1838, and offered again and almost accepted in 1840, resisting a strong temptation to accompany Sir James Ross to the Antarctic regions on the scientific exploring expedition of the "Erebus" and "Terror" in 1840–41, and regretfully giving up the idea of a Scottish professorship, which, during his early years of residence in Lincoln's Inn, had many attractions for him, he finally made the bar his profession. But during all the long years of hard work, through which he gradually attained to an important and extensive practice, and to a high reputation as a Chancery barrister, he never lost his interest in science, nor ceased to be actively engaged in scientific pursuits; and he always showed a lively and generous sympathy with others, to whom circumstances (considered in this respect enviable by him) had allotted a scientific profession.

About the year 1841 his attention was drawn to the problem of ships' magnetism by his friend Major Sabine, who was at that time occupied with the reduction of his own early magnetic observations made at sea on board the ships "Isabella" and "Alexander," on the Arctic Expedition of 1818, and of corres-

ponding magnetic observations which had been then recently made on board the "Erebus" and "Terror" in Captain Ross's Antarctic Expedition of 1840–41. The systematic character of the deviations, unprecedented in amount, experienced by the "Isabella" and "Alexander" in the course of their Arctic voyage, had attracted the attention of Poisson, who published in 1824, in the "Memoirs of the French Institute," three papers containing a mathematical theory of magnetic induction with application to ships' magnetism. The subsequent magnetic survey of the Antarctic regions, of which by far the greater part had to be executed by daily observations of terrestrial magnetism on ship-board, brought into permanent view the importance of Poisson's general theory; but at the same time demonstrated the necessity for replacing his practical formulæ by others, not limited by certain restrictions as to symmetry of the ship, which he had assumed for the sake of simplicity. This was the chief problem first put before Smith by Sabine, and his solution of it was the first great service which he rendered to the practical correction of the disturbance of the compass caused by the magnetism of ships.

In 1850 he published separately an account of his theoretical and practical investigations on the correction of the deviations of a ship's compass, which was afterwards given as a supplement to the Admiralty "Practical Rules" in 1855. The large deviations found in iron-plated ships of war "having rendered necessary the use of the exact instead of the approximate formulæ," this article was rewritten by Smith for the Compass Department of the Admiralty. It now forms Part III. of the "Admiralty Manual for the Deviations of the Compass," edited by Evans and Smith, to which are added appendices containing a complete mathematical statement of the general theory, proofs of the practical formulæ, and constructions and practical methods of a more mathematical character than those given in the body of the work for ordinary use. A separate publication, of "Instructions for Correcting the Deviation of the Compass," by Smith, was made by the Board of Trade in 1857.

It is satisfactory to find that the British Admiralty "Compass Manual," embodying as it does the result of so vast an amount of labour, guided by the highest mathematical ability and the

most consummate practical skill, has been appreciated as a gift to the commonwealth of nations by other countries than our own. It is adopted by the United States Navy Department, and it has been translated into Russian, German, Portuguese, and French. Smith's mathematical work, and particularly his beautiful and ingenious geometrical constructions, have attracted great interest, and have called forth fresh investigation in the same direction, among the well-instructed and able mathematicans of the American, Russian, French, and German Navy Departments.

The constancy to the compass problem, in which Smith persevered with a rare extreme of disinterestedness, from the time when Sabine first asked him to work out practical methods from Poisson's mathematical theory, until his health broke down two years before his death, was characteristic of the man. It was pervaded by that "ténacité passionée" which a generous French appreciation describes as a peculiarity of the English nation; but there was in it also a single-mindedness and a purity of unselfishness to be found in few men of any nation, but simply natural in Archibald Smith.

Honourable marks of appreciation reached him from various quarters, and gave him the more pleasure from being altogether unsought and unexpected. The Admiralty, in 1862, gave him a watch. In 1864 he received the honorary degree of LL.D. from the University of Glasgow. The Royal Society awarded to him the Royal Medal in the year 1865. The Emperor of Russia gave him, in 1866, a gold compass, emblazoned with the Imperial Arms and set with thirty-two diamonds, marking the thirty-two points. Six months before his death Her Majesty's Government requested his acceptance of a gift of L.2000, as a mark of their appreciation of "the long and valuable services which he had gratuitously rendered to the Naval service in connection with the magnetism of iron ships, and the deviations of their compasses." The official letter intimating this, dated Admiralty, July 1st, 1872, contains the following statement, communicated to Smith by command of the Lords of the Admiralty:—"To the zeal and ability with which for many years you have applied yourself to this difficult and most important subject, My Lords attribute in a great degree the accurate information they possess in regard to the influence of magnetism, which has so far conduced to the safe navigation of iron

ships, not only of the Royal and Mercantile Navies of this country, but of all nations."

In private life those who knew Archibald Smith best loved him most; for behind a reserve which is perhaps incident to engrossing thought, especially when it is concerned with scientific subjects, he kept ever a warm and true heart; and the affectionate regrets of his friends testify to the guileless simplicity and sweetness of his disposition, which nothing could spoil or affect. About the close of 1870 he was compelled by ill-health to give up work, but two years later he had wonderfully rallied; and though he was not strong enough to resume his legal or scientific work, he was able to take his old interest in his boys' mathematical studies. A few weeks before his death he revised the instructions for compass observations to be made on board the "Challenger," then about to sail on the great voyage of scientific investigation now in progress; and he spoke several times of the satisfaction it gave him to feel able again to do such work without effort or fatigue. The attack of illness which closed his life was unexpected and of but a few hours' duration. In 1853 he married a daughter of Vice-Chancellor Sir James Parker, then deceased, and he leaves six sons and two daughters. He died on the 26th of December 1872.

The following Gentlemen were elected Fellows of the Society:—

A. Forbes Irvine, Esq.
Benjamin Carrington, M.D., Eccles, Lancashire.
William Ferguson, F.L.S., F.G.S.,
T. B. Sprague, M.A. Cantab.
Thomas Muir, M.A.
J. Batty Tuke, M.D., F.R.C.P.E.
William Durham, Esq.

Monday, 16th February 1874.

Sir W. THOMSON, President, in the Chair.

The following Obituary Notices of Deceased Fellows of the Society were read :—

1. Obituary Notice of the Very Rev. Dean Ramsay. By the Rev. D. F. Sandford.

The Very Reverend Edward Bannerman Ramsay, Dean of the Diocese of Edinburgh, in the Episcopal Church of Scotland, was born in Aberdeen on the 31st day of January 1793. His father was Sir Alexander Ramsay, Bart., of Balmain and Fasque. Sir Alexander was the second son of Sir Thomas Burnett, Bart., of Leys, but had assumed the name of Ramsay, and been created a Baronet, on succeeding to the estates of his maternal uncle in Forfarshire. He was by profession an advocate at the Scottish bar, and Sheriff of his native county of Kincardine. In that county the family of Burnett of Leys have held lands and a high position for many hundred years. Bishop Burnet of Salisbury, the historian of his own times, and a divine of enlarged mind and liberal views, belonged to it. The Bishop's picture, in his robes as Chancellor of the Order of the Garter, is among the family portraits at Crathes Castle, the seat of the present Sir James Burnett, Bart.

The Dean's mother was Elizabeth, eldest daughter and co-heiress of Sir Alexander Bannerman, Bart., of Elsick, a lady of considerable personal attractions and marked character. She and her husband were in Paris at the outbreak of the great French Revolution. They escaped from France under the protection of a tricolour cockade worn by the Sheriff, which Dean Ramsay presented some years ago, as an interesting relic of the time, to the Antiquarian Museum in Edinburgh. On reaching Scotland they settled at Aberdeen, and so Edward Bannerman, their fourth son, who was born soon after, first saw the light in his own ancestral country. This was always a subject of deep gratification to one whose whole heart and sympathies were so eminently Scottish. In early life Edward Ramsay was sent to reside with

his great-uncle, the then Sir Alexander Ramsay, who placed him at school in a small village near his own residence, Harlsey, in Yorkshire. The locality was a very retired one, and old customs lingered there which time had changed or obliterated in other parts of England. The Bible lay chained to the desk in the parish church, as in the days of Edward VI. and Queen Elizabeth. The bodies of the deceased were carried to the quiet churchyard by those of their own sex, age, and condition. The village girls bore their companions, the boys their schoolfellows, the young men and women, the middle-aged and the old, their contemporaries and associates who had been called away. The parish curate dined with the squire every Sunday, but did not omit to drink to the health of the old butler who waited at table, as well of his host, and the other guests. The village carpenter, a strange character, forestalled Archbishop Whately's historic doubts as to the existence of Napoleon Buonaparte, and boldly declared that he did not believe there was any such person. His conviction was that the name was used to frighten children, and to terrify the British nation into keeping up the army and navy, and paying the very heavy taxes imposed upon them. From this primitive spot, where doubtless his powers of observation and his interest in local peculiarities were first awakened by the circumstances just mentioned, which he never forgot, Ramsay was transferred to the Grammar School at Durham. Here, as he often stated with regret, he was taught little and learnt less. After leaving Durham, he was a pupil for a short time of Dr Joynes, a clergyman at Sandwich in Kent, and then entered St John's College, Cambridge, where he took his degree in 1816. Although not distinguished in any remarkable way as a scholar or mathematician in the University, Mr Ramsay seems to have felt satisfied with the result of more than one of the College examinations, and he obtained during his residence at St John's a scholarship on that learned foundation. Within a very short period after taking his degree, he received a title for holy orders as curate of Rodden in Somersetshire; and was ordained by the Bishop of Bath and Wells, Deacon in December 1816, and Priest in the following year. When acting in after life as examiner of candidates for the ministry, he frequently drew a comparison between the meagre superficial examinations,

confined to a paper on the Evidences of Christianity, and a few verses of the Greek Testament, to which he was subjected, and the more thorough and searching ordeal through which aspirants to the clerical office are now required to pass. He continued at Rodden for seven years, perhaps in some respects the happiest in his life. Although his rector was non-resident, he was allowed to conjoin the care of the neighbouring parish of Buckland with that of Rodden, and to discharge also for a time the duties of evening lecturer in the parish church of Frome. This afforded to him another contrast in his own remembrance with the present requirements as to residence, experience, and work on the part of the clergy. While at Rodden, he employed his leisure time and annual holiday in the study of botany, making more than one expedition into Wales and elsewhere with this object. He also gave some time to the cultivation of music, for which he had considerable talent. And he seems also to have turned his attention to mathematics and astronomy, incited thereto by his brother, the late Admiral Sir William Ramsay, who gave him a box of instruments and a telescope, which he used in the instruction of a class of young friends and parishioners.

After declining the offer of an appointment to a chapel in his native city, Aberdeen, Mr Ramsay came to Edinburgh, at the end of 1823, as curate to Mr Shannon, the incumbent of St George's Episcopal Chapel in York Place. This change of residence introduced him to Edinburgh at a time when not only agitation for political and municipal reform, but also the awakening of religious thought and feeling to which the Clapham School had given rise in England, and which was soon to merge in the remarkable Oxford movement of 1833, were intermingling with its intellectual culture and social life. The refined, cultivated, and earnest-minded young clergyman, possessing hereditary claims to be received among the highest circle of its inhabitants, soon established also close and intimate relations with many of those who then made our city so distinguished. He became popular in the best sense of the word. His ministrations and preaching were highly appreciated. His kindly pleasing manners and unaffected genuine character won for him an influence which was soon felt for good in many quarters. After serving the curacy of St George's

for two years, Mr Ramsay was appointed incumbent and pastor of the interesting old chapel and genuine Scottish Episcopalian congregation of St Paul's, Carrubber's Close, in the Old Town. The chapel was largely attended during his ministry, and the value of the living while he held it was L.400 per annum.

In 1827 he was appointed assistant minister of St John's, and, on the death of the late Bishop Sandford in 1830, was elected to the incumbency of that charge, which he continued to hold until his long and honoured life reached its close on the 27th December 1872. The more strictly professional details and characteristics of Mr Ramsay's career are not subjects of comment or notice in this place. It will suffice to mention that in the faithful and assiduous discharge of his duties he secured to himself appreciation, confidence, and esteem, which, as years rolled on and in proportion as he became better known, grew and ripened into genuine and universal regard and love.

In 1838 he proposed and carried through the General Synod of the Scottish Episcopal Church a canon for establishing a society, the main object of which was to supplement the very inadequate stipends of the clergy, to provide teachers for the poor, and generally to improve the financial condition of the Communion to which he belonged. He was specially useful as a catechist among the young of his flock, and compiled a manual of catechetical instruction for their use, which has passed through more than twelve editions. He published a volume of Advent Sermons, also pastoral letters addressed to his congregation on various subjects, occasional sermons and pamphlets on matters connected with his own communion, and a series of Lectures on Diversities of Character, and another series on Faults in Christian Believers, which were subsequently combined and expanded into a Treatise on the Christian Life. In 1841 Mr Ramsay was appointed by Bishop Terrot, on his own elevation to the Episcopate, Dean of the Diocese of Edinburgh. In 1845 he was offered by Sir Robert Peel, on behalf of the Crown, the Bishopric of New Brunswick in Nova Scotia, and in 1848, and again in 1862, he was elected by the clergy of two Scottish dioceses to be their Bishop. But he saw fit to decline on each of these occasions the offer of a mitre, much to the satisfaction of his own congregation, who viewed with little favour these

attempts to deprive them of their tried and valued friend and pastor. In 1859, on the occasion of the installation of his distinguished friend, Mr Gladstone, as Lord Rector, the University of Edinburgh conferred on the Dean the degree of LL.D. In 1828–1829, he was one of the secretaries of the ordinary meetings of the Royal Society. He subsequently became a member of Council, and in 1859 a Vice-President. In 1861 he opened the winter session with an address from the chair, which was published in the Proceedings. The only paper contributed by him to the Society's General Transactions was a biographical memoir of the late Dr Chalmers, with whom he was on terms of intimate friendship. A few years ago he inaugurated a movement for erecting a statue of the same eminent philosopher and divine, which is now approaching completion in the studio of Mr John Steele, and is to be placed at the intersection of George Street and Castle Street in this city.

The Dean's continued interest in botanical study was evinced by his publishing a notice of the works and discoveries of his friend Sir J. E. Smith. His taste for the highest style of music, and his earnest desire to extend the knowledge and cultivation of it, led him to choose, as the subject of two lectures before the Philosophical Institution of Edinburgh, "The Genius and Works of Handel." They were delivered to a crowded audience in the Music Hall, with the assistance of illustrations by a choir, and were afterwards published. The Dean delivered before the same body a lecture on Pulpit Oratory and Orators, and pursued the subject thus suggested in a printed letter to a young clergyman on the art of clear and articulate public speaking, in which he was himself an unsurpassed proficient. The work, however, with which his name is most widely connected is his "Reminiscences of Scottish Life and Character." It has gone through twenty editions, and more than ninety thousand copies of it have been sold. It is to be found on the library tables of royalty and in the cottage of the peasant. It is sold by the newsboys at every railway station. It is to be seen in the huts of new settlements in Western America, and of the cattle and sheep runs of New Zealand and Australia. It has made the Dean's good Scottish name a household word in every land on which the sun shines. Wherever the exiled Scotchman goes, he carries with him the

"Reminiscences" as one of the links which will continue to bind his heart to his own country, and to keep alive in his memory the most vivid and pleasing recollections of her history and people. The object of the book was not to produce a mere momentary amusement, but to contribute to an important branch of historical science, the neglect of which has left us too ignorant of what our forefathers and their times really were. It was intended to preserve the remembrance of old Scottish customs, and of national peculiarities and characteristics, the traces of which, in many respects to our loss, are fast dying out. That jocular sayings and anecdotes should prove the staple of the work was an accident, or rather I might say a necessity, and not an arbitrary choice of the author. It may have its literary faults. But many of us were too partial to the man, too much in sympathy with his purpose and with the genuine, kindly, patriotic motives which guided his pen, to dwell on them. Nay more, critics have been slow to point them out, and the judgment of the public has done more than condone them. It may not be too much to apply to the "Reminiscences" the language which the greatest Scottish novelist has used with regard to his own works, and to say that the Dean was happy in the knowledge that the perusal of his book has amused hours of relaxation and relieved those of languor, pain, and anxiety, and that it has contributed in no small degree to the happiness and instruction of his fellow-countrymen. It is no little credit, the Dean felt it to have been a great privilege, to have followed, however humbly, in the footsteps of Sir Walter Scott, and to have added to the literature of his country a volume which must always serve to make Scotland better known, appreciated, and loved, wherever it is read.

We may not intrude into the sacred domestic circle to find material for this biographical notice; it may suffice to say that those who knew Dean Ramsay best loved him best. He was honoured above most men with the friendship of the good and great. Dignified ecclesiastics, eminent statesmen, nobles of character and renown no less exalted than their rank, sought and valued his acquaintance, his wise counsels, his kindly sympathy. Men of distinction and repute from all quarters found a welcome under his roof, and never left it without feeling that they had added to their circle of life-long friends.

In every philanthropic movement the Dean was ready to assist with his money and influence. He gave largely, from no great means, to charitable agencies and to individual cases of need and distress. He was a friend to those of every class with whom he was brought in contact. The cabmen vied with each other as to which of them should take him for his daily drive, and they counted his presence at more than one of their yearly social entertainments a special honour. Every one seemed gratified at any occasion for intercourse with him, even for a few moments. He was essentially a gentleman, dignified, courteous, and kindly. The Dean's influence in his own Communion was deservedly very great, and if it was exerted in every way in his power to advance her usefulness and prosperity, it was at the same time always tempered and guided by a spirit of charity and good-will, which enabled him to do more than almost any man of his day and generation to purify and sweeten the atmosphere both of social and ecclesiastical life in this city and country. Whatever estimate may be formed of the views he held, the work he did, this at least must be universally admitted, and may not unfitly be put on record even here. To Dean Ramsay, charity, freedom from bigotry, narrowness, and ill-will, were not the accidents of temperament, or the fruits of an easy disposition, of high breeding, and culture. They were essential elements in the ideas he had formed of the Christian religion and of the Christian character. He was never tired of enforcing them in his teaching, as he never ceased to illustrate and exemplify them in his conduct. And that his endeavours to do this by every means he could, and towards men of every creed and party known to the religious and political world, were acknowledged and appreciated, the great demonstration which took place at his funeral amply testified. It was not only in numbers one of the largest which ever took place in this city, but it was attended by the leading representatives, both lay and clerical, of every denomination. Men forgot their differences and the causes of their separation one from another, as they gathered round his grave. It was the realisation for once of the dream and aspiration of Dean Ramsay's own life. It was the most striking and worthy tribute which could possibly have been paid to his memory.

It will be well for religion, as, I may venture to add, it will be well also for learning, and science, and truth in all its forms and aspects, if the same spirit which breathed and spoke in all that Dean Ramsay did and said shall increase, and spread, and deepen among us, in our various spheres and callings. We cannot but feel that in every point of view Dean Ramsay's was a career which, as it was honoured while he was spared to us, and marked by such distinctions as befitted his position in the Church and in society during his life, so it demanded some tribute and notice in this place, now that his name is withdrawn from the roll of our living Fellows. If it was not given him to further the cause of science and learning, as many belonging to the Royal Society have done, yet his teaching and example were such as all may profitably recall to memory and strive to follow and imitate.

2. Obituary Notice of Professor Rankine. By Lewis D. B. Gordon, C.E.

William John Macquorn Rankine was the son of Lieutenant David Rankine of the Rifle Brigade, younger son of Macorne or Macquorn Rankine, Esq., of Drumdow in Ayrshire, and thus of an ancient Scottish family. His mother was the elder daughter of Archibald Grahame, Esq., of Drumquhassel. He was born in Edinburgh, 5th July 1820. Rankine records of himself, "My earliest distinct recollection is that of my mother teaching me the Lord's Prayer, next my father explaining to me the character of Jesus Christ;" and further he records, "My early instruction in arithmetic and elementary mechanics and physics was mainly obtained from my father." The mutual dependency thus begun continued through as beautiful a life of mutual self-devotion between parents and son as can be pictured; for the three were rarely far separate during the fifty years the parents lived after his birth.

Rankine went to the Ayr Academy in 1828, and afterwards to the High School of Glasgow in 1830, and thence to Edinburgh, where he studied geometry under Mr George Lees; but his knowledge of the higher mathematics was chiefly obtained by private study. He records that in 1834 "My uncle Archibald Grahame

gave me a copy of 'Newton's Principia,' which I read carefully; this was the foundation of my knowledge of the higher mathematics and dynamics and physics." He read the Principia in the original Latin, and in after life recommended his pupils so to read this work of paramount authority and reputation; "for," said he, "modern science has added no new principle to the dynamics of Newton; what it has done is to extend the application of dynamical principles to phenomena to which they had not been previously applied; in fact, to the correlation of the physical sciences—or, in other words, what is denoted by the convertibility of energy." Thus, at the early age of fourteen, had Rankine begun to discipline his mind and train his analytical powers on Newton's model of unquestionable definition and exhaustive demonstration, characteristics of the many works on cognate subjects he was himself in after years to contribute for the education of engineers of every class, and for the advancement of physical science. For two years, from 1836 to 1838, Rankine was a student in the University of Edinburgh, and took the courses of Natural Philosophy, Chemistry, Natural History, and Botany. He continued for two sessions under Professor Forbes; and the first year gained the gold medal for "An Essay on the Undulatory Theory of Light," and the extra prize (gold medal) for "An Essay on Methods in Physical Investigation." At this period, too, he read much metaphysics, chiefly Aristotle, Locke, Hume, Stewart, Degerando. The whole tendency of his mind was to the digestion and assimilation of the highest human knowledge. But the *res angusta domi* demanded that he should take a profession; and at this period none was more in vogue, or apparently more promising of abundant employment, than that of a civil engineer.

Rankine having for a short time assisted his father, who was superintendent of the Edinburgh and Dalkeith Railway, in 1838 became a pupil of Mr M'Neil (afterwards Sir John M'Neil), whose practice in Ireland was varied and extensive. Accordingly, for four years Rankine was actively employed as a pupil on various surveys and schemes for river improvements, water works, and harbour works, and on the Dublin and Drogheda Railway. While on this work, he contrived and practised a method of "setting out curves" by chaining and angles at the circumference, since

known as *Rankine's method.* He was much loved and respected by his numerous fellow-pupils, several of whom have attained high professional status. His pupilage ended, Rankine returned to Edinburgh, and was occupied for some time in the preparation and publication of an "Experimental Inquiry into the Advantages attending the Use of Cylindrical Wheels on Railways."

The theoretical investigation, and the deductions from the results of the experiments, conducted by his father and himself, are characterised by the same completeness in every respect as his more important and more famous writings of maturer years. But cylindrical wheels never came into use. It was "too late" to begin an obvious improvement, or there was no time to think of it; and yet, taking everything into consideration, the wheels would be better cylindrical, so formed that they should retain that shape for the longest time.

In 1842–43 various papers were sent to the Institution of Civil Engineers, and prizes were granted for them. There is one on "The Fracture of Axles," in which the importance of continuity of form and fibre was first shown, and the hypothesis of spontaneous crystallisation disproved. The conclusions of this paper were generally accepted and acted upon in the construction of axles.

In 1844–45, and afterwards till 1848, Rankine was employed under Messrs Locke and Errington on various railway projects promoted by the Caledonian Railway Company, of which his father had become secretary. But from 1842 onwards his mind had been much occupied in perfecting himself in the use of the higher analysis and in its application to the mechanics of molecular vortices.

Rankine's first investigation of the principles of the mechanical action of heat appeared in a paper received by the Royal Society of Edinburgh in December 1849, and published in their Transactions, vol. xx. It is based on what he calls "the hypothesis of molecular vortices;" that is to say, the supposition that the motions of which Davy showed thermotic heat to consist are of the nature of vortices—whirls or circulating streams. This is the part of the hypothesis that is specially connected with the phenomena of the mechanical action of heat; but in order to connect these with some other phenomena, Rankine makes the

further suppositions that the whirling motion is diffused in the form of atmospheres round nuclei, which may be either bodies of a special kind or centres of condensation and attraction in the atmospheres; and that radiance, whether of heat or light, consists in the transmission of a vibratory motion of the nuclei, by means of forces which they exert on each other.

The *quantity of heat* in a body is the energy of its molecular vortices; the *absolute temperature* of the body is the same energy divided by a specific co-efficient for each particular substance. A *perfect gas* is a substance in which the elastic pressure is sensibly that which varies with the centrifugal force of the vortices only; and the intensity of the pressure, according to the known principles of mechanics, must be proportioned directly to the energy of the vortices, and inversely to the space that they occupy. In substances not *perfectly gaseous*, the elasticity is modified by attractive or cohesive forces. When the deviation from the perfectly gaseous state is small, the effects of such forces may be approximately represented by series, in terms of the reciprocal of the absolute temperature. Rankine had previously published an example of the use of such series, in a paper on the Elasticity of Vapours (Edin. Phil. Journal, July 1849), and he also applied them with success to the elasticity of carbonic acid and some other gases (Phil. Mag. 1851). *Sensible heat* is the energy employed in varying the velocity of the whirling particles; *latent heat* the work done in varying the dimensions of their orbits, when the volumes and figures of the spaces in which they whirl are changed. The force which keeps any particle in its orbit is equal and opposite to the centrifugal force of that particle; therefore the work done in varying the orbits of the particles is proportionate to their centrifugal forces, therefore to the energy of the vortices, therefore to the *absolute temperature.* And to compute that quantity of work, or latent heat, when a body undergoes a given variation of dimensions, the *absolute temperature* is to be multiplied by the corresponding variation of a certain function of the dimensions and elasticity of the body. This function is computed by taking the rate of variation with temperature, of the external work done during the kind of change of dimensions under consideration.

Such is an outline of the method by which Rankine deduces

the second law of thermodynamics, or general equation of the mechanical action of heat, from the hypothesis of molecular vortices, by means of known dynamical principles. The quantity whose variation being multiplied by the absolute temperature gives the latent heat, corresponding to a given change of dimensions at that temperature, is expressed in Rankine's earlier papers by symbols, but is not designated by a special name.

In a paper read in January 1853 (Edin. Trans. xxi) he proposes the name *Heat Potential*; and in a paper read to the Royal Society of London, January 1854, he gives to the same quantity, with a certain additional term, depending on changes of temperature, the name of "Thermodynamic Function,"—a name which has since been adopted by various other authors.

In Rankine's paper of 1849, the chief applications of the general equation of thermodyamics are as follow:—The values of *apparent* as distinguished from *real specific heat*, for gases and vapours under various circumstances. The demonstration that the apparent specific heat of a vapour kept constantly at the pressure of saturation, while its volume varies, is *negative* for most fluids at ordinary temperature—in other words, that steam, for example, tends to become *partially liquified when it works expansively*, contrary to what had been previously believed. This fact was first verified experimentally by M. Hirn of Colmar. And the demonstration that the total heat of evaporation of a perfect gas increases with temperature at a rate equal to the completed specific heat of the gas at *constant pressure*.

In the paper read December 1850, he deduced from *Joule's Equivalent* the value 0·24 for the specific heat of air, and concluded that the previously received value 0·2669 must be erroneous. This was exactly verified by Regnault's experiments, but not till more than three years afterwards.

In a paper read April 1851 (Edin. Trans. vol. xx. 205) he deduced from the general equation of thermodynamics, as given in his paper of 1849, the following law of the efficiency of a perfect heat engine,—that the *whole heat expended is to the heat which disappears in doing mechanical work, as the absolute temperature at which heat is received to the difference between the temperatures at which it is received and rejected.*

In Rankine's paper of 1849, groups of circular vortices were supposed to be arranged in spherical layers round the atomic nuclei, in order to simplify the investigation. On the 18th December 1851, he read a paper (Edin. Trans. xx. p. 425) in which it was shown that precisely the same results as to the relations between heat, elasticity, and mechanical work, follow from the supposition of molecular vortices of any figure arranged in any way. In a long series of papers he applied the principles of thermodynamics to various practical equations relating to the steam-engine and other heat engines, and he was the author of the first separate treatise in which the science of thermodynamics was set forth with a view to its practical application (A Manual of the Steam-Engine and other Prime Movers, 1859). In two papers read to the Philosophical Society of Glasgow in 1853–1855 respectively, he pointed out how the laws of thermodynamics and of electro-dynamics might be regarded as particular cases of general laws applicable to *energy* in the abstract, and especially to transformation between the two great classes of "actual and potential" energy.

Clausius, who, it is well known, discovered the second law of thermodynamics consentaneously with Rankine, having taken occasion in 1866 to lay great weight on his having adopted no special hypothesis on the molecular constitution of bodies, but to have deduced the second law from general principles, Rankine, in an address to the Philosophical Society of Glasgow, concluded an eloquent justification of the mechanical hypothesis of molecular vortices in these words :—"I wish it to be clearly understood that although I attach great value and importance to sound mechanical hypothesis as means of advancing physical science, I firmly hold that they can never attain the certainty of observed facts; and accordingly, I have laboured assiduously to show that the two laws of thermodynamics are demonstrated as facts independent of any hypothesis; and in treating the practical application of those laws, I have avoided all reference to hypothesis whatsoever."

In March 1854 he was awarded the Keith medal of the Royal Society of Edinburgh for the researches above summarised, mostly in his own words. His name and fame had become European. He was elected Fellow of the Royal Society of London, and con-

tributed to that Society many papers of permanent interest in the course of the next sixteen years.

From January to 20th April 1855, Rankine lectured for Professor Gordon at Glasgow College, on "Applied Mechanics" and the "Application of Thermodynamics to the Theory of the Steam-Engine." These lectures were of so high a character of usefulness, and delivered in so masterly a manner, that steps were immediately taken to get Rankine appointed to the professorship on the resignation of Mr Gordon. The Queen's commission appointing him Regius Professor of Civil Engineering and Mechanics was dated November 7, 1855.

On the 3rd of January 1856 he delivered his introductory lecture "On the Harmony of Theory and Practice in Mechanics," an essay full of practical wisdom. In November 1856 the introductory lecture "On the Science of the Engineer," was delivered, and concludes thus:—"Let the young engineer then be convinced that the profession which he studies is not a mere profitable business, but a liberal and a noble art, tending towards great and good ends, and that to strive to the utmost to perfect himself in that art, and in the sciences on which it depends, is not merely a matter of inclination or of policy, but a sacred duty."

Rankine's whole career as a professor exemplified this view of the profession of an engineer. By efforts, which to ordinary men seem altogether impossible, he published in rapid succession four manuals of "Mechanical and Engineering Science and Practice," on the best models for arrangement, but original in the treatment of many subjects,—always lucid in definition and demonstration, and replete with applications to examples of the practice of experienced men in all departments.

The students of engineering during the previous existence of the Professorship had gradually awakened to the necessity of acquiring some preliminary scientific instruction, and Rankine's style of teaching at once incited them to far higher efforts. It is unquestionable that his scientific works generally, and his manuals of applied science especially, have done more to break down the long persistent fallacy of a discrepancy between rational and applied mechanics, between theory and practice in engineering, than any previous publications whatever, and the influence of his systematic

scientific teaching is spreading true principles of engineering design in this country, as the works of Navier, Poncelet, Morin, and Weisbach had done many years previously on the engineering practice of France and Germany. I say advisedly, that in far fewer cases now-a-days do we see the strength and stability which ought to be given by the skilful arrangement of the parts of the structure, supplied by means of an imposing massiveness involving a lavish expenditure of material and labour—that is, money—than twenty years ago was usual.

His complete knowledge of foreign languages enabled him to correspond with such men as Weisbach, Zeuner, Verdet, and other professors of applied mechanics on the Continent, to the mutual interest and advantage of all. He also corresponded in German with Poggendorf, Clausius, and Helmholz. Each of the manuals has gone through many editions,—that on the "Steam Engine," &c., nine; "Applied Mechanics," seven, and so on.

In 1862 he effectually called the attention of the Senate of the University to the manner in which the usefulness of the Chair of Civil Engineering and Mechanics was impaired through its being isolated from other branches of study, and induced the authorities to establish a systematic curriculum of study and examination in all the sciences bearing on engineering, followed by the granting of certificates to the successful candidates; a measure which led to a steady and continuous increase in the number and efficiency of the students in the engineering department of the University; and it could, indeed, scarcely be otherwise, seeing that William Thomson taught Natural Philosophy, and Rankine taught its applications. His style of lecturing was attractive; he never failed or faltered in an exposition or demonstration; and his power of illustration of the details of steam-engine practice, for example, was unusually lucid from his knowledge of the chemistry of the subject being co-extensive with his mechanical and physical knowledge. He at once gained the confidence of thoughtful students, and during the first session, that in which he lectured for Professor Gordon, he contracted an intimacy with Mr J. R. Napier, a shipbuilder and engineer, ambitious to emancipate his business from being that of one of mere empiricism, and this friendship, as it ripened, proved of great consequence to the whole science of shipbuilding and steam propulsion.

In 1856 he first projected a treatise on shipbuilding, which he ultimately finished in 1866, and published in conjunction with J. R. Napier and others. Of this treatise it may be said it is unique of its kind. It has recently been published in German.

In the autumn of 1857 he contrived a theory of skin resistance of ships, based on experiments furnished by J. R. Napier, and in the next year applied it with complete success to the steam-ship "Admiral," verifying his theory.

The work on shipbuilding occupied much of his spare time. He records at several intervals, from 1863 to 1866, brief notes, such as "Working hard at Treatise on Shipbuiling," "Researches on Neöids," "Stream lines." In 1866 the folio treatise was published. Rankine wrote the greater portion of it, and was the editor. The preparation of this treatise led to a series of researches on fluid motion, which are acknowledged to be of the highest importance, and they certainly belong to the most abstruse parts of mathematical science. Rankine's genius overcame all difficulties, and the "Theory of the Propagation of Waves," the "Theory of Waves near the Surface of Deep Water," and his investigations on plane water lines in two dimensions, *i.e.*, of the lines of motion of water flowing past a ship, advanced, in his hands, the application of science to naval architecture as much as his discovery of the second law of thermodynamics did that of the theory of the steam-engine and other heat engines. For, the practical use of his theory of oögenous water-lines reproduces *known* good forms of water-line, and even reproduces their numerous *varieties*, which differ very much from each other. In fact, there is no form of water-line that has been found to answer in practice which cannot be imitated by means of oögenous neöids—that is, ship-shape curves generated from an oval.

Besides Mr J. R. Napier, the late John Elder was the intimate friend of Rankine, and the bold improvements introduced by that distinguished engineer in marine steam machinery were constantly discussed with Rankine, whose scientific aid in insuring success was gracefully and munificently acknowledged by Elder's widow, by the gift of a large endowment to increase the emoluments of the chair of Civil Engineering and Mechanics.

Rankine's professional business was that of a consulting engi-

neer, and in this capacity he made several reports to his clients of permanent value. One, "On Canal Haulage," is of great interest, and another "On the Explosion of the Tradeston Flour-Mills."

He was consulting engineer of the Highland Society of Scotland.

This sketch of the leading incidents of the scientific works which have made Rankine's name and fame represents, though very feebly, the more permanent portion of his usefulness to his profession and to his generation. But besides these great works, he contributed about 150 papers of greater or less importance to philosophical journals, mechanics' magazines, and to "The Engineer" in particular; generally expositions of such questions as the day or week suggested connected with engineering and mechanics; and it has been truly said—"With him thought was never divorced from work, both were good of their kind; the thought profound and thorough—the work a workmanlike expression of the thought." "Few, if any, practical engineers have contributed so much to abstract science, and in no case has scientific study been applied with more effect to practical engineering."

Rankine was a steady attendant at the meetings of the British Association, and took an active part as President of Section G, or Secretary of Section A, or otherwise in these meetings, where he had a universal acquaintance, and was universally respected and esteemed. He was a member of the "Red Lion's" Club.

In 1857 he took the most active part in founding the "Institution of Engineers in Scotland." He was the first President. It has proved a successful and eminently useful institution.

The outward lustre of Rankine's career is of course derived from his scientific work, but there was an inner halo surrounding him, which to his friends shone even brighter than the outward lustre. He was a true gentleman, gentle, chivalrous, self-forgetting, and scrupulously truthful, a patient listener, a quiet expounder. He supported applause without feeling the weakness of vanity. He had not a vestige of the spirit of rivalry, being of a thoroughly genial temperament. In his judgment of other men he obeyed the pious injunction of Thomas à Kempis, "Ad hanc estiam pertinet, non quibuslibet hominum verbis credere, nec audita vel credita mox ad aliorum aures effundere."

His health for several years in his early youth was feeble, and he

occupied himself much with the theory of music, and practised the piano and violoncello. Though too much occupied in after life to allow of his attaining much proficiency, he could always interest and amuse his friends by singing his own songs to his own music, always gay and cheery. "The Coachman of the Skylark" in 1854, "The Engine-driver's Address to his Engine" in 1858, and "The Mathematician in Love," and "The Three-foot Rule," somewhat later, had a grotesque gracefulness of humour which were irresistible. His appearance was highly prepossessing, as the Fellows of the Royal Society of Edinburgh well know. His social qualities were the admiration of his acquaintances and the delight of his friends. Full of anecdote and information, he was an ornament to society, of which he was always the least obtrusive member, but often the centre of attraction. Everything he did he did well. Singing, croquet, or bézique, he used to join in them cordially, and intent on the moment's amusement.

His first great grief was the death of his father in May 1870. Rankine's affectionate and devoted nature was deeply moved, and he himself began soon after to experience symptoms of decay of his hitherto vigorous health. When, in April 1871, his excellent mother died, he was for a time quite absorbed by his grief for her loss. His own health became more and more unsatisfactory. Especially his eyesight became very weak, and during 1872 he had to employ an amanuensis and an assistant in his class work, one of his pupils, M. Bamber.

He visited his more intimate friends much during the summer of this year, where he could enjoy rest, and quiet, and amusement. But his health gradually gave way, and towards the end of November his medical friends perceived that the great mind of Rankine was giving way.

On the 24th December he died, leaving a noble record of genius to future generations, and a sweet memory to those of his contemporaries who knew him personally.

3. Obituary Notice of Justus Liebig. By Professor Crum Brown.

Justus Liebig was born on the 12th May 1803, at Darmstadt, where his father carried on business as a grocer and colour merchant. He early showed a strong inclination to the study of experimental chemistry, reading all the chemical books he could procure from the Darmstadt Library, and repeating every experiment he read of, as far as he could obtain from his father's warehouse necessary materials. His father acceded to his wish that he should be a chemist, and as the only way in which this could be carried out, sent him at the age of fifteen to an apothecary's shop to learn chemistry. There he remained only ten months, and he returned to Darmstadt satisfied that he must seek some other mode of obtaining his object. He remained at home for some months preparing for a University course, upon which he entered in 1819 at Bonn. He soon left Bonn for Erlangen, where he studied chemistry under Kastner. When at Erlangen he attended Schelling's lectures, and long after used to speak of the interest he had taken in them, and of the injurious effect they had exercised upon his success as a practical investigator. Both at Bonn and at Erlangen he founded a students' society of chemistry and physics, in which the members communicated and discussed novelties of science. Liebig left Erlangen in 1822, having already published a paper on the preparation of Schweinfurth green.

Assisted by the liberality of the Grand Duke Louis of Hesse, he proceeded to Paris, where he attended the lectures of Gay-Lussac, Thenard, and Dulong, and obtained from Gay-Lussac permission to work in his private laboratory. He there carried on his investigation into the composition and properties of the fulminates, the results of which he communicated to the Academy. He at once attracted the notice of Humboldt, who was then resident in Paris, and through his influence was appointed, in 1824, Extraordinary Professor of Chemistry in the University of Giessen. In 1826 he was raised to the ordinary professorship. In 1845 the Grand Duke of Hesse conferred upon him the title of Baron von Liebig. In 1852 he accepted an invitation by the Bavarian Government

to the ordinary Professorship of Chemistry, and the Directorship of the Chemical Laboratory in the University of Munich. He died 18th April 1873, at Munich.

The time had not yet come for a calm and judicial estimate of Liebig's influence on the progress of chemistry. It must be left for future generations of chemists, removed from the direct influence of his work, and unbiassed by personal recollection, to assign him his proper place among the great leaders of chemical thought and investigation. It is, however, possible for us to give a general sketch of his career, and to point out some of the more prominent effects of his work as seen in the present state of the science.

We may consider him as a teacher of chemistry, as an inventor of new means of investigation, as a discoverer of new facts and a creator of new ideas in pure chemistry, and as an expounder of the relations of chemistry to common life and to the arts. As a teacher, he introduced into Germany systematic practical training in laboratory work, and induced the Darmstadt Government to build at Giessen a students' laboratory, which has served as the type of those magnificent scientific laboratories which have recently been erected in connection with all the great German universities. His stinging attacks upon the great German Governments for their neglect of practical scientific education, his own success as a teacher, and the zeal for the good cause which he imparted to his pupils, have had for their effect the establishment throughout Germany of numerous well-equipped and usefully active schools of practical science. It is not too much to say that there is no school of chemistry in the world which does not owe a great part of its usefulness to the example of the Giessen laboratory.

It is unnecessary here to catalogue the improvements in chemical apparatus which we owe to Liebig, but there is one invention which must at once occur to every chemist as of vital importance in the history of the science. Organic analyses were made with great accuracy before 1831, but they could be made only by highly skilled chemists, and involved great labour and trouble. The publication by Liebig, in that year, of his method of organic analyses—the method which (with important but secondary improve-

ments) we still employ, made it easy for any advanced student to make an accurate analysis of an organic body. It may be truly said that the astonishingly rapid development of organic chemistry, which dates from that time, was only rendered possible by the simplification of the method of organic analysis entirely due to Liebig.

Of Liebig's discoveries and speculations it is possible to give, in such a notice as this, only an outline. The whole progress of chemistry for the last fifty years is so intimately connected with what he did, that a life of Liebig would necessarily include the history of chemistry for that period.

His investigations extend to nearly every branch of chemistry, but it was to organic chemistry that he specially devoted himself; and it is through his work, in this direction chiefly, that he has influenced other departments of chemistry and the science generally. His first research, that on fulminic acid, published in Paris in 1823, led to the recognition of the isomerism of fulminic acid and the cyanic acid discovered in 1822 by Wöhler, and was followed by a long series of investigations on the compounds related to cyanogen, in which he opened out and to a great extent explored this intricate and interesting path of inquiry. Another group of researches was directed to the determination of the composition and constitution of organic acids. In a comprehensive memoir published in 1838, he pointed out the analogies between many organic acids. and phosphoric acid, and introduced the idea of polybasic acid into organic chemistry, enumerating the criteria for the determination of the basicity of an acid with extraordinary precision and accuracy.

He made numerous analyses of the vegetable alkaloids, and greatly increased our knowledge of their properties, of their equivalents, and of the relation of equivalent to composition.

His investigations into the derivatives of alcohol, particularly those formed by oxidation and by the action of chlorine, including the discovery of aldehyde and chloral, poured a flood of light upon the whole question of the constitution of organic compounds. Liebig was the first to regard ether as an oxide, of which alcohol is the hydrate, and the compound ethers salts. By doing so he challenged the defenders of the "etherine" theory, who looked upon

ether as a hydrate of olefiant gas. The result was one of those controversies which have proved of immense value in the progress of chemistry. In the course of this controversy the relations of alcohol and ether to other substances were investigated and discussed with great minuteness, and the result was the general adoption of Liebig's ethyl theory. The subject of decay, putrefaction, and fermentation early engaged Liebig's attention. Entirely opposed to the *vital* theory of fermentation, he attacked it with both argument and ridicule, and proposed a purely chemical theory, which he defended with great ingenuity.

A very important part of Liebig's work in pure organic chemistry was carried on along with Wöhler. As might be expected, the joint efforts of two men of such genius and industry produced results unexampled in number and importance. One of the first objects of their research (in 1830) was cyanic acid, a substance discovered by Wöhler, and in which Liebig had a special interest from its isomerism with his fulminic acid. But the investigations undertaken by them, which exercised the greatest influence on the science of chemistry were those on the benzoic compounds and on uric acid. These are models of what such work ought to be, not only enriching the science with new facts, but compacting it by the discovery of new relations. The theoretical views brought forward in the papers on benzoic acid and bitter almond oil were the commencement of the development of the new theory of compound radicals which soon took the place of that of Berzelius.

The most widely known part of Liebig's work consists in his applications of chemistry to physiology and agriculture. The facts he discovered in reference to the chemistry of animal and vegetable nutrition, and the explanations he gave of the chemical processes involved in the life of organisms, have had an incalculable effect upon physiological chemistry. In his application of the principles of chemistry to agriculture, he proceeded in a thoroughly scientific manner; and although he in some cases generalised too fast, and was thus led into practical error, his work forms the foundation of a true science of agriculture.

By far the greater part of Liebig's scientific work was done at Giessen. After his removal to Munich, the claims of society and the court life of a capital upon his time made the devotion to

laboratory work which distinguished the earlier part of his career impossible. His work in Munich consisted chiefly in elaborations of his previous ideas, and in researches, the results of which are of comparatively little general scientific interest, although in some cases of considerable practical value. Among these may be mentioned the discovery of the mode of preparing the extract of meat, and that of a method of depositing a uniform coherent layer of silver of any thickness upon smooth surfaces.

Liebig was a most voluminous author. His papers were published in many journals, but chiefly in Poggendorff's "Annalen," and in the "Annalen der Pharmacie" (now "Justus Liebig's Annalen der Chemie und Pharmacie"), of which he became one of the editors in 1831. Of separately published books, the most important are "Introduction to the Analysis of Organic Bodies," 1837; "Chemistry in its Application to Agriculture and Physiology," 1840; "Animal Chemistry," 1842; "Handbook of Organic Chemistry" (as second volume of a revised edition of Geiger's "Pharmacy"), 1843; "Chemical Letters," 1844; "On the Chemistry of Food," 1847; "On Some Causes of the Motions of the Juices in the Animal Body," 1848; "Principles of Agricultural Chemistry, with special Reference to the late Researches made in England," 1855. Of most of these works many editions were published in German and in almost every European language. From 1831 till his death he was one of the editors of the chemical journal now known as "Justus Liebig's Annalen der Chemie und Pharmacie." Along with Kopp he edited, from 1847 to 1856, the "Jahresbericht über die Fortschritte der Chemie;" and along with Poggendorff and Wöhler, the "Handwörterbuch der Chemie."

His personal character was simple and easily characterised. Open, amiable, and generous, vehement in carrying out his convictions, utterly intolerant of pretence and dishonesty, he was either a warm friend or a declared enemy. In controversy he was often violent, sometimes ferocious, but he never struck an unfair blow.

By his death many chemists have lost a friend, and all feel one more link attaching them to the last generation broken.

4. Obituary Notice of Gustav Rose. By Professor Crum Brown.

Gustave Rose was born in Berlin on the 18th of March 1798. He was the youngest son of the pharmaceutical chemist, Valentin Rose, and the brother of Heinrich Rose, the eminent analytical chemist. He intended to devote himself to mining engineering, and began his practical studies in Silesia; but in consequence of illness gave up this profession, and occupied himself with scientific chemistry and mineralogy. He studied mineralogy under Weiss, in the University of Berlin, and made a large number of careful measurements of crystals. His first published work was his graduation thesis, "De Sphenis atque Titanitæ systemate Crystallino," 1820.

Like many young chemists of his time, he was attracted to Stockholm, where he studied under the guidance of Berzelius, the greatest and most accurate chemist of that age, and by frequent excursions in Sweden made himself thoroughly acquainted with the varied mineralogy of that country. In Stockholm he met Mitscherlich, with whom he maintained a life-long friendship. Late in life he felt it necessery for him to explain, which he did in a friendly and modest way, the share he had in the work which led to Mitscherlich's discovery of isomorphism. In 1823 he became lecturer on mineralogy in the University of Berlin; in 1826 he received the title of extraordinary Professor; and in 1849 was appointed ordinary Professor of Mineralogy and Director of the Mineralogical Collections.

Rose travelled much in search of mineralogical knowledge. He visited England, Scotland, Scandinavia, Italy, and France, studying rocks, mines, and museums; and in 1829 was selected by Humboldt as one of his companions in his examination of the Ural and Altai Mountains. There Rose discovered many new minerals, and in a special work, "Reise nach dem Ural," 1837 and 1842, made known the remarkable mineral wealth of that part of the Russian empire. His holidays were usually occupied by excursions in Silesia or in the Harz, where he collected the materials for some of his most valuable investigations. During one of his walks in Silesia he sustained an injury of the knee, from which he suffered much, but continued

his lectures till the 11th July 1873, when he was attacked with inflammation of the lungs, from the effects of which he died on the 15th of July.

His most important works were an elaborate memoir on felspar (1823); numerous investigations on quartz, on granite, on the metals which crystallise in rhombohedra; on the conditions under which carbonate of lime crystallises in the form of calcspar, or in that of arragonite, on meteorites, and on the mineralogical constituents of trap-rocks. Besides these purely mineralogical researches, special interest attaches to his study of the relation between the crystalline form and the physical properties of minerals. He pointed out that in tourmaline and in electric calamine the pyro-electric polarity is connected in a constant manner with the crystalline polarity, and described with great minuteness the forms of these minerals.

In 1857 Marbach showed that the crystals of iron pyrites and also those of cobaltine, both minerals crystallising in forms belonging to the regular system, could be divided into two sets, differing extremely in thermo-electric character, the one set more positive than antimony, the other more negative than bismuth. Rose saw at once that this difference must be related to their crystalline form, and that these two sets must possess crystalline characters of a right and left handed kind, and at last succeeded in detecting the difference between them.

Most of his researches were published in "Poggendorff's Annalen," in the Transactions of Berlin Royal Academy of Sciences, and in the Journal of the German Geological Society. Besides the "Reise nach dem Ural," already mentioned, he published a short work on the "Elements of Mineralogy," distinguished by beautifully drawn figures, and one on a crystallo-chemical system of classification of minerals.

Professor Rammelsberg, from whose notice of Rose's life most of the foregoing sketch has been taken, testifies to the remarkable kindliness and geniality of his character, to the pleasure which he felt in the success of his young scientific friends, and to his hatred of polemical discussion.

5. Obituary Notice of the Rev. Professor Stevenson, D.D. By John Small, M.A., Librarian to the University of Edinburgh.

Professor William Stevenson was born at Barfod, in the parish of Lochwinnoch, on the 26th October 1805. His father was the proprietor of a small estate called Broadfield, and William was his second son. He entered the University of Glasgow in 1821, and pursued his studies at that University during the usual curriculum in the Faculty of Arts, with the exception of one session (1824–25) which he spent at St Andrews, attracted by the popularity of Dr Chalmers, who was at that time Professor of Moral Philosophy there. While at the University of Glasgow he attended diligently to his studies, and worked particularly for the classes of mathematics and natural philosophy. During the summer months he acted as tutor in the family of the late Mr Cochran of Ladyland, and thus began a friendship which lasted uninterruptedly till the time of his death. It was the arranging and cataloguing the old library at Ladyland that developed the love of books for which he was afterwards so remarkable, and the catalogue he then made is still carefully preserved. He pursued his theological studies at the University of Glasgow, but was in session 1828–29 at the University of Edinburgh. In theology he was a distinguished student, in some sessions carrying off the highest honours. After finishing his university course, he was licensed by the Presbytery of Paisley on the 5th of May 1831.

He officiated for six months in the Presbyterian Church in Limerick in 1832, and in July 1833 was appointed by the Crown assistant and successor to the Rev. George Gleig, minister of Arbroath, on whose death two years afterwards he succeeded to the charge.

While at Arbroath Mr Stevenson enjoyed the friendship of the Rev. Dr Thomas Guthrie, then minister of Arbirlot, and an amusing account is given in the autobiography of that eminent divine, of a public discussion with the Rev. Dr Ritchie, "the Goliath of Voluntaryism," held at Arbroath, in which Mr Stevenson took a prominent part (vol. i. p. 167). The account of the discussion on this occasion was published in the form of a pamphlet, with the

following title: "Account of a Meeting held at Arbroath on the 16th April 1834, in Defence of Church Establishments, with a full Report of the Speeches delivered on that occasion by the Rev. Messrs Stevenson, Meek, Whitson, Lee, Guthrie, and Muir." This publication attracted considerable attention, and brought the speakers prominently before the public; one of them was the Rev. Dr Robert Lee, afterwards Professor of Biblical Criticism, then minister of a Chapel-of-Ease at Inverbrothock. In 1839 Mr Stevenson's health gave way, and he suffered so much from chronic bronchitis that he had to spend the winter of that and the following year at Torquay.

On the re-establishment of his health, Mr Stevenson was in 1844 presented by the Crown to the first charge of the parish of South Leith. This valuable preferment enabled him to gratify his intense love of reading, and he collected rare and valuable books, not only on theology, but on every subject illustrating the history and antiquities of Scotland. In 1848 he was elected a Fellow of the Society of Antiquaries of Scotland, and in 1849 he received the degree of D.D. from the University of Edinburgh.

Whilst minister of South Leith Dr Stevenson took much interest in his parochial duties, and in 1851 published a small volume, entitled "Christianity and Drunkenness." He was also a contributor to Macphail's "Edinburgh Magazine," and the topics he handled were "The Buchanites," "Pusey and the Confessional," and matters relating to the great Gorham controversy in the Church of England. He took part in the proceedings of the General Assembly, and was appointed Convener of the Colonial Committee in 1859.

In 1858 he was elected a Fellow of the Royal Society of Edinburgh.

In 1861 he was appointed to the Chair of Divinity and Ecclesiastical History in the University, on the death of the Rev. Dr Robertson. As Professor, his method of conducting the class was somewhat peculiar. In place of giving in each session a simple outline of his very extensive subject, he chose rather to take a limited period in the Church's history, and illustrate this in the most minute manner. Every heresy or controversy that had cropped up in the period selected received due attention, and was

illustrated by extracts from rare works which he had collected for the purpose. In his first session (1861-62), the period embraced in his lectures was only from A.D. 30 to 100.

Although the Government, when he was appointed Professor of Church History, dissociated from the Chair the valuable appointment of Secretary to the Bible Board for Scotland, still Dr Stevenson, from his private resources, was enabled to gratify to the utmost his passion for adding to his library. He was a member of the Bannatyne Club and other literary societies originated for printing valuable historical manuscripts, ancient poetry, &c., &c., and the recondite works he thus received were not in his case put hastily on his shelves, but were carefully read and criticised.

He was Vice-President of the Society of Antiquaries for several years, and, as his colleague, Sir James Simpson, had given a great impetus to archæological matters in Scotland, Dr Stevenson went with him hand in hand. His reminiscences of excursions ("howking expeditions" as they were called) planned by Sir James to places of antiquarian interest in the neighbourhood of Edinburgh, were very amusing.*

As Dr Stevenson, from his excellent scholarship, desired extreme minuteness and accuracy in every literary work, this may account for his published writings being fewer than his abilities led his friends to expect. But, while it was supposed that his appoint-

* One of these, arranged in honour of Dr Reeves, of Trinity College, Dublin, was to inspect the curious buildings still existing at Culross, and Sir James had chartered a steamer to take a large party from Leith to visit that ancient Burgh. So much time, however, had been lost in visiting Inch Garvie and other islets in the Firth, that on reaching Culross, from the shallowness of the water, the steamer had to anchor a long way from the shore. Nothing daunted, Sir James, with a dozen of followers, got into a small boat, but it at last ran aground. The rest of the party getting into another boat, and avoiding the error the first had committed, reached the pier by a circuitous route, and lent their aid to bring the party which had first left the steamer ashore. They were at last carried through the shallow water and mud on the shoulders of the Culross boatmen, and the appearance of Sir James himself as he was supported on the backs of two sailors, with other two lending their assistance, created great merriment This was often referred to by Dr Stevenson as one of his happiest excursions. Although differing in Church politics, a great friendship existed between Sir James and Dr Stevenson, and on the death of the former Dr Stevenson was much affected. He expressed his feelings in a poem, a portion of which was inserted in the Life of Sir James by the Rev. Dr Duns.

ment to the Chair of Church History might have allowed him leisure to publish some results of his extensive reading and matured thought, the plan he had laid down for teaching the history of the Church (as before observed) necessitated the writing of new lectures for each year. In this way he sometimes wrote seventy new lectures in one session. In any intervals of leisure, however, he enjoyed miscellaneous reading, and sometimes indulged in poetical effusions. He translated into verse the Latin rhymes in the well-known Aberdeen Breviary, which he printed, but did not then complete.

As ancient Scottish literature, especially poetry, had always been a favourite subject with him, Dr Stevenson was often consulted about the publication of manuscript remains of our early Doric vernacular, and several works of this kind, when they appeared, were dedicated to him. In 1870 he took much interest in an edition of the works of Gavin Douglas, the poetical Bishop of Dunkeld, then projected. He read over the proof sheets, and aided in expiscating some circumstances attendant on the double consecration of that ancient Scottish Prelate. About the same time he resolved to complete the legends from the Aberdeen Breviary, by appending historical notes, and they at length appeared in an octavo volume about the end of 1872. The title of the work is as follows :—" The Legends and Commemorative Celebrations of St Kentigern, his Friends and Disciples, translated from the Aberdeen Breviary and the Arbuthnot Missal, with an illustrative Appendix. Printed for private circulation, 1872."

In the preface, he states that at one time he had in view " to prepare a complete calendar of the Scottish Saints, and, taking the national legends of the Aberdeen Breviary for a basis, he proposed to intercalate all that he might be able to ascertain regarding those *Dii minores* of our country's earlier faith, who, although not enrolled in that dignified service book, are mentioned in other literary monuments now less recondite than they were then, or have left some dim memories of themselves in the names of the towns, villages, fairs, and wells of our country, sometimes in remote and lonely districts, or spots where there had once been chapels, cells, or hermitages."

The want of leisure prevented his carrying out so extensive a

plan, but we are indebted to him for some interesting information regarding the group of saints more immediately connected with the Lothians and Fife, viz., St Kentigern, and his mother St Thenew (daughter of Loth, King of the Lothians), St Servanus or St Serf, St Columba, St Asaph, St Baldred of the Bass, St Conwall, and St Palladius.

From his family connection with Clackmannanshire he was much attached to that district, and for several summers he occupied a villa in the neighbourhood of Muckart. In this retirement he was always happy, surrounded by his family, and supplied with the newest literature. One season was distinguished by some rural festivities, which he commemorated in verse in a tiny volume printed in 1872 ("The Yetts o' Muckart; or the Famous Pic-nic and the brilliant Barn-Ball. In hairst auchteen hunder an' seventy-one.")

Finding his health failing, Dr Stevenson, with much reluctance, resigned his Chair in November 1872.

This step was much regretted by his colleagues, and his retirement was gracefully referred to by Principal Sir Alexander Grant, in his opening address of the College, session 1872–73, and in the introductory lectures of his colleagues in the Faculty of Theology, who all expressed the hope that he would enjoy the rest to which he was so well entitled.*

The good wishes of the learned Principal and the Professors were not realised, and the last year of Dr Stevenson's life was spent in much annoyance from the effects of an accident he had

* The allusion by Sir Alexander Grant to Dr Stevenson was in the following terms:—"I regret now to have to announce the retirement, owing to impaired health, of Dr Stevenson, who for eleven years has occupied the important Chair of Divinity and Ecclesiastical History. During that time Professor Stevenson has shown himself to be a man of real learning; he has exhibited that quality which the philosopher Coleridge used to value highly, and which he called 'book-mindedness.' In an age distracted by a number of ephemeral interests, and, at the same time vaunting itself on a Baconian adhesion to things rather than to words, this quality of 'book-mindedness,' the characteristic of the scholar of the olden times, has a tendency to become rare. But, for the interests of humanity, it is necessary that there should be not only men who study nature, but also men whose life is spent in books—whose minds are more taken up with the past than the present; to whom everything suggests an association with some great writer, and who thus

the misfortune to receive some years previously; but, enfeebled as he was, he spent any intervals from suffering in preparing additional notes to his work on St Kentigern (which had been very favourably noticed), in the event of an edition being published after his demise. [It is believed that this edition is nearly ready for publication.]

Till within a few days of his death he was able to see his friends, and at last died peaceably on the 14th of June 1873, in the 68th year of his age.

Dr Stevenson was twice married, and left issue by both marriages.

6. Obituary Notice of Auguste De la Rive. By Professor George Forbes.

Auguste De la Rive, one of our foreign Honorary Fellows, was born in the year 1801. He resided principally at Geneva, where for a long time he held a professorial chair. He made journeys in various European countries, and spent a considerable time in England and Scotland. After a long and active life, he was struck down by paralysis. A severe attack of gout added to his infirmity. The death of numbers of his friends and relatives deeply affected him. His state of health rendered it desirable that he should winter in the south of France in 1873. He died at Marseilles on the 27th November 1873, at the age of 72 years. His faculties were not impaired by infirmities, and up to the year of his death he continued to communicate memoirs to the Physical Society of Geneva.

M. De la Rive was chiefly interested in the study of electricity. In the Royal Society catalogue we find 106 articles, chiefly on this

serve as the living interpreters of libraries, and as links to maintain the hereditary succession of thought. Such a man as this is our friend Professor Stevenson, and such a character as his is the appropriate ornament of Universities. He has ever manifested not only the learning, but also the urbanity, of the true scholar; and in quitting the labours of the class-room and the Senate-hall to seek that repose which has now become necessary to him, he will not leave a single enemy behind. He will take with him into privacy the regrets of his colleagues, and their sincere wishes that he may yet enjoy many years of happiness and peace."

subject, written by himself, besides 10 in company with others. Since the date of that catalogue he added to the number. The first paper of importance written by him was published in the year 1822, and contained many ingenious and important experiments illustrative of the discoveries of Oërsted and Ampère. His interest in chemistry led him to espouse the chemical theory of the voltaic current. On seven different occasions he supported this view in various scientific journals. His researches on electro-chemical decomposition were in part the basis of the modern art of electro-plating. He made several experimental inquiries into the heat generated by the passage of electricity through conductors; some of his most celebrated and original experiments had reference to the action of magnetism upon the electric discharge. These experiments led him to form a theory of the aurora, on which subject he published a series of articles from the year 1848 to the year 1862. In 1862 he illustrated the theory by a number of beautiful experiments publicly exhibited at Geneva. At various epochs he discussed historically the progress of electrical science.

But the work of M. De la Rive was not confined to electricity. In the years 1838–39 he discussed the phenomenon of sunset, usually called the second coloration of Mont Blanc; and his explanation is now generally adopted. He made experiments on specific heats; and his communications on the variations of terrestrial magnetism, as depending upon elevation above and depression below the surface of the soil, are of considerable value. Some of his latest researches had reference to Faraday's discovery of the magneto-rotary effect of bodies upon plane-polarised light. He was a great friend of Faraday's, of whose life he wrote an interesting review, published in the "Bibliothèque Universelle."

Auguste De la Rive exerted himself to spread an interest in science among those with whom he came in contact. His genial manner and his open hospitality gathered round him a large circle of friends. He always extended a helping hand to the young man of science. Many could bear witness to this trait in his character; and it was well illustrated by the manner in which he welcomed Faraday, and discovered his talent, at a time when the coldness of Sir Humphrey Davy would have led many to neglect him.

Most of the scientific societies of Europe bestowed upon M. De

la Rive the title of Honorary Member. The Royal Society of London elected him a Foreign Member. He was also a Corresponding Member of the Academy of Sciences at Paris.

7. Obituary Notice of Dr J. Lindsay Stewart. By Dr Cleghorn, Stravithy.

Dr Stewart was a native of Kincardineshire, and obtained his medical education in Glasgow. After graduating he proceeded in 1856 to the Presidency of Bengal as assistant-surgeon; he was present at the siege of Delhi in 1857, and in 1858 he joined the expedition to the Yuzufzai country. In 1860–61 he officiated for Dr W. Jameson as superintendent of the Botanic Garden, Saharunpore. His position gave him an excellent opportunity of becoming acquainted with the vegetation of the Terai and North-West Himalaya, and afterwards at Bijnour he studied the Flora of Rohilkund, and of the valleys between the Ganges and Sardah. As Conservator of the Forests of the Punjab (1864), his duties took him to all parts of that province, and also to Sindh, Kashmir, and the inner Himalayan tracts on the Upper Indus, Chenab, and Sutlej rivers, which adjoin Turkistan and Tibet. During his journeys, under the most difficult circumstances, he maintained his habit of taking copious notes, and accumulated an immense store of information regarding the plants of North-West India. The results of these researches are embodied in numerous papers published in the Journals of the Royal Geographical Society, the Asiatic Society of Bengal, the Agri-Horticultural Society of India, and the Transactions of the Botanical Society of Edinburgh. A most interesting account of the vegetation of the extreme north-west corner of the Punjab and the hills beyond it, which he studied during the Yuzufzai campaign, is contained in his "Memoranda on the Peshawur Valley, chiefly regarding its Flora" (*Journ. As. Soc.*, 1863), and in his "Notes on the Flora of Waziristan" (*Journ. Roy. Geo. Soc.*, 1863). In the "Journal of the Agri-Horticultural Society of India" appeared "The Sub-Sevalik Tract, with special reference to the Bijnour Forest and its Trees" (vol. xiii. 1865); "Journal of a Botanising Tour in Hazara and Khagan" (vol. xiv. 1866); and "A Tour on the Punjab Salt Range" (vol. i. new series, 1867). His last

communication, "Notes of a Botanical Tour in Ladak or Western Tibet," appeared in the "Transactions of the Botanical Society of Edinburgh" (vol. x. 1869). In 1869, after twelve years of unremitting labour, mental and bodily, Dr Stewart returned to England, and the Government of India entrusted him with the preparation at Kew of a Forest Flora of Northern and Central India. To this great work Dr Stewart devoted a large part of his furlough, and he would doubtless have completed it in a satisfactory manner if his health had not given way. That this was the cause became apparent on his return to India, when, after a few months of office work, sickness obliged him to move from Lahore to the Hill Sanitarium at Dalhousie, where he died on 5th July 1873, aged forty-one.

8. Obituary Notice of John Hunter. By J. T. Bottomly, Esq., The University, Glasgow.

Mr John Hunter was born in Belfast on the 23d of March 1843. He was the only son of the late Dr Hunter of Belfast, a gentleman who, though he was for many years before his death unable to move, was highly esteemed as a consulting physician. Mr Hunter, till he entered Queen's College, Belfast, received his education chiefly at home. During his undergraduate course he was distinguished in nearly every branch of science; and in 1863 he obtained the degree of B.A. in the Queen's University, with first-class honours in Chemistry and Experimental Physics. With similar distinction he took the degree of M.A. the following year. In the interval he held the Senior Scholarship in Chemistry in Belfast, a scholarship which is competed for annually by Bachelors in Arts of the Queen's University; and it was during this year that he published his first paper on the "Absorption of Gases by Charcoal." In 1865 he became assistant to Dr Andrews, the Professor of Chemistry in Queen's College, Belfast, an office which he held till 1870, when he was elected Professor of Mathematics and Natural Philosophy in King's College, Windsor, Nova Scotia. At Windsor his health suffered severely from the climate; and, feeling unable to encounter a second winter, he resigned his professorship, and returned home in the autumn of 1871.

During the winter of that year he took up his residence at Enniscrone, county Mayo, being under medical advice to give up active work for some months at least; but with a strong desire to carry on his chemical researches, he fitted up for himself a temporary laboratory there; and he was actively engaged in prosecuting them at the time of his sudden death, on the 13th of September 1872. His death was occasioned by an acute disease of the brain, of which he seems to have had a slight warning some months previously; but his last illness was not more than a few hours of intense pain.

He was married in 1869. His wife survives him; but he left no children.

Mr Hunter's researches were chiefly concerned with the absorption of gases by charcoal. He examined a large number of charcoals, and came to the conclusion that the greatest absorptive power is possessed by the dense charcoal of the shell of the cocoa-nut. With this material he proceeded to examine the absorption of a very large number of gases and vapours; and he extended his researches to the absorption of mixed vapours. He also investigated the relation between absorption and temperature in the cases of ammonia and cyanogen, and showed that, while raising the temperature at which the charcoal is exposed to, the gas decreases the absorption in both cases; the rate of decrease is much greater in the case of ammonia than in the case of cyanogen, between 0° C. and 55° C.; but at 55° C. the rate of decrease in the case of ammonia suddenly diminishes, and up to 80° C. it is not very much greater than the rate of decrease for cyanogen. At a point a little higher than 55° the volumes absorbed are the same for the two gases. Above this point more of cyanogen gas is absorbed by a given weight of charcoal than of ammonia; but below that point ammonia is enormously more absorbed than cyanogen. Mr Hunter was extending his observations to the effect of pressure on absorption. He had already published two papers on the subject. The last of these was communicated to the Chemical Society of London only a few weeks before his death; and it is in fact scarcely complete, through wanting his final corrections in type on it.

Mr Hunter accompanied the Deep Sea Dredging Expedition in H.M.S. "Porcupine" in the autumn of 1869, and published two

important papers "On the composition of Sea-water collected at different depths of the Atlantic, from a few feet below the surface up to 2090 fathoms," and "On the Composition of the Atlantic Ooze." These analyses included also analysis of the absorbed gases of the water. His papers are all published in the "Journal of the Chemical Society."

He was genial, warm-hearted, affectionate, a universal favourite with those who knew him, enthusiastically devoted to science, and withal highly cultivated in literature and the arts. His premature death, at a time when a life of usefulness seemed to have just commenced, is deeply regretted.

The following statement respecting the Fellows of the Society was submitted:—

I. Honorary Fellows—		
Royal Personage, ...	1	
British Subjects, ...	17	
Foreign, ...	32	
Total Honorary Fellows, . . .		50
II. Non-resident Member under the Old Laws, . .		1
III. Ordinary Fellows—		
Ordinary Fellows at November 1872, ...	343	
New Fellows—William Boyd, Esq.; Donald Crawford, Esq.; Dr John G. M'Kendrick; M. M. Pattison Muir, Esq; Dr J. Bell Pettigrew; Andrew Pritchard, Esq.; Walter Stewart, Esq.; Robert Tennent, Esq.; Robert Walker, Esq.; Dr Morrison Watson; Robert Wilson, Esq.; Major Welsh, ...	12	
W. Dittmar, Esq., reinstated, ...	1	
		356
Carried forward,		356

Brought forward,		356
Deduct Deceased—Rev. Dr Guthrie; Prof. John Hunter; Very Rev. Dean Ramsay; Prof. Macquorn Rankine; Arch. Smith, Esq.; Rev. Prof. Stevenson; Dr J. L. Stewart; R. W. Thomson, Esq., ...	8	
Resigned—J. F. M'Lennan, Esq.; Dr Alex. Wood,	2	
Cancelled—Dr Richardson, Dr Foulerton, . .	2	
		12
Total number of Ordinary Fellows at Nov. 1873,		. 344

The following Communications were read:—

9. The Kinetic Theory of the Dissipation of Energy. By Sir William Thomson.

In abstract dynamics the instantaneous reversal of the motion of every moving particle of a system causes the system to move backwards, each particle of it along its old path, and at the same speed as before, when again in the same position. That is to say, in mathematical language, any solution remains a solution when t is changed into $-t$. In physical dynamics this simple and perfect reversibility fails, on account of forces depending on friction of solids; imperfect fluidity of fluids; imperfect elasticity of solids; inequalities of temperature, and consequent conduction of heat produced by stresses in solids and fluids; imperfect magnetic retentiveness; residual electric polarisation of dielectrics; generation of heat by electric currents induced by motion; diffusion of fluids, solution of solids in fluids, and other chemical changes; and absorption of radiant heat and light. Consideration of these agencies in connection with the all-pervading law of the conservation of energy proved for them by Joule, led me twenty-three years ago to the theory of the dissipation of energy, which I communicated first to the Royal Society of Edinburgh in 1852, in a paper entitled "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy."

The essence of Joule's discovery is the subjection of physical phenomena to dynamical law. If, then, the motion of every par-

ticle of matter in the universe were precisely reversed at any instant, the course of nature would be simply reversed for ever after. The bursting bubble of foam at the foot of a waterfall would reunite and descend into the water; the thermal motions would reconcentrate their energy, and throw the mass up the fall in drops re-forming into a close column of ascending water. Heat which had been generated by the friction of solids and dissipated by conduction, and radiation with absorption, would come again to the place of contact, and throw the moving body back against the force to which it had previously yielded. Boulders would recover from the mud the materials required to rebuild them into their previous jagged forms, and would become reunited to the mountain peak from which they had formerly broken away. And if also the materialistic hypothesis of life were true, living creatures would grow backwards, with conscious knowledge of the future, but no memory of the past, and would become again unborn. But the real phenomena of life infinitely transcend human science, and speculation regarding consequences of their imagined reversal is utterly unprofitable. Far otherwise, however, is it in respect to the reversal of the motions of matter uninfluenced by life, a very elementary consideration of which leads to the full explanation of the theory of dissipation of energy.

To take one of the simplest cases of the dissipation of energy, the conduction of heat through a solid—consider a bar of metal warmer at one end than the other, and left to itself. To avoid all needless complication, of taking loss or gain of heat into account, imagine the bar to be varnished with a substance impermeable to heat. For the sake of definiteness, imagine the bar to be first given with one-half of it at one uniform temperature, and the other half of it at another uniform temperature. Instantly a diffusing of heat commences, and the distribution of temperature becomes continuously less and less unequal, tending to perfect uniformity, but never in any finite time attaining perfectly to this ultimate condition. This process of diffusion could be perfectly prevented by an army of Maxwell's "intelligent demons,"* stationed at the

* The definition of a demon, according to the use of this word by Maxwell, is an intelligent being endowed with free-will and fine enough tactile and perceptive organisation to give him the faculty of observing and influencing individual molecules of matter.

surface, or interface as we may call it with Professor James Thomson, separating the hot from the cold part of the bar. To see precisely how this is to be done, consider rather a gas than a solid, because we have much knowledge regarding the molecular motions of a gas, and little or no knowledge of the molecular motions of a solid. Take a jar with the lower half occupied by cold air or gas, and the upper half occupied with air or gas of the same kind, but at a higher temperature, and let the mouth of the jar be closed by an air-tight lid. If the containing vessel were perfectly impermeable to heat, the diffusion of heat would follow the same law in the gas as in the solid, though in the gas the diffusion of heat takes place chiefly by the diffusion of molecules, each taking its energy with it, and only to a small proportion of its whole amount, by the interchange of energy between molecule and molecule; whereas in the solid there is little or no diffusion of substance, and the diffusion of heat takes place entirely, or almost entirely, through the communication of energy from one molecule to another. Fourier's exquisite mathematical analysis expresses perfectly the statistics of the process of diffusion in each case, whether it be "conduction of heat," as Fourier and his followers have called it, or the diffusion of substance in fluid masses (gaseous or liquid), which Fick showed to be subject to Fourier's formulæ. Now, suppose the weapon of the ideal army to be a club, or, as it were, a molecular cricket bat; and suppose. for convenience, the mass of each demon with his weapon to be several times greater than that of a molecule. Every time he strikes a molecule he is to send it away with the same energy as it had immediately before. Each demon is to keep as nearly as possible to a certain station, making only such excursions from it as the execution of his orders requires. He is to experience no forces except such as result from collisions with molecules, and mutual forces between parts of his own mass, including his weapon. Thus his voluntary movements cannot influence the position of his centre of gravity, otherwise than by producing collision with molecules.

The whole interface between hot and cold is to be divided into small areas, each allotted to a single demon. The duty of each demon is to guard his allotment, turning molecules back, or allowing them to pass through from either side, according to certain

definite orders. First, let the orders be to allow no molecules to pass from either side. The effect will be the same as if the interface were stopped by a barrier impermeable to matter and to heat. The pressure of the gas being, by hypothesis, equal in the hot and cold parts, the resultant momentum taken by each demon from any considerable number of molecules will be zero; and therefore he may so time his strokes that he shall never move to any considerable distance from his station. Now, instead of stopping and turning all the molecules from crossing his allotted area, let each demon permit a hundred molecules chosen arbitrarily to cross it from the hot side; and the same number of molecules, chosen so as to have the same entire amount of energy and the same resultant momentum, to cross the other way from the cold side. Let this be done over and over again within certain small equal consecutive intervals of time, with care that if the specified balance of energy and momentum is not exactly fulfilled in respect to each successive hundred molecules crossing each way, the error will be carried forward, and as nearly as may be corrected, in respect to the next hundred. Thus, a certain perfectly regular diffusion of the gas both ways across the interface goes on, while the original different temperatures on the two sides of the interface are maintained without change.

Suppose, now, that in the original condition the temperature and pressure of the gas are each equal throughout the vessel, and let it be required to disequalise the temperature, but to leave the pressure the same in any two portions A and B of the whole space. Station the army on the interface as previously described. Let the orders now be that each demon is to stop all molecules from crossing his area in either direction except 100 coming from A, arbitrarily chosen to be let pass into B, and a greater number, having among them less energy but equal momentum, to cross from B to A. Let this be repeated over and over again. The temperature in A will be continually diminished and the number of molecules in it continually increased, until there are not in B enough of molecules with small enough velocities to fulfil the condition with reference to permission to pass from B to A. If after that no molecule be allowed to pass the interface in either direction, the final condition will be very great condensation and very low temperature in

A; rarefaction and very high temperature in B; and equal temperature in A and B. The process of disequalisation of temperature and density might be stopped at any time by changing the orders to those previously specified (2), and so permitting a certain degree of diffusion each way across the interface while maintaining a certain uniform difference of temperatures with equality of pressure on the two sides.

If no selective influence, such as that of the ideal "demon," guides individual molecules, the average result of their free motions and collisions must be to equalise the distribution of energy among them in the gross; and after a sufficiently long time, from the supposed initial arrangement, the difference of energy in any two equal volumes, each containing a very great number of molecules, must bear a very small proportion to the whole amount in either; or, more strictly speaking, the probability of the difference of energy exceeding any stated finite proportion of the whole energy in either is very small. Suppose now the temperature to have become thus very approximately equalised at a certain time from the beginning, and let the motion of every particle become instantaneously reversed. Each molecule will retrace its former path, and at the end of a second interval of time, equal to the former, every molecule will be in the same position, and moving with the same velocity, as at the beginning; so that the given initial unequal distribution of temperature will again be found, with only the difference that each particle is moving in the direction reverse to that of its initial motion. This difference will not prevent an instantaneous subsequent commencement of equalisation, which, with entirely different paths for the individual molecules, will go on in the average according to the same law as that which took place immediately after the system was first left to itself.

By merely looking on crowds of molecules, and reckoning their energy in the gross, we could not discover that in the very special case we have just considered the progress was towards a succession of states, in which the distribution of energy deviates more and more from uniformity up to a certain time. The number of molecules being finite, it is clear that small finite deviations from absolute precision in the reversal we have supposed would not

obviate the resulting disequalisation of the distribution of energy. But the greater the number of molecules, the shorter will be the time during which the disequalising will continue; and it is only when we regard the number of molecules as practically infinite that we can regard spontaneous disequalisation as practically impossible. And, in point of fact, if any finite number of perfectly elastic molecules, however great, be given in motion in the interior of a perfectly rigid vessel, and be left for a sufficiently long time undisturbed except by mutual impact and collisions against the sides of the containing vessel, it must happen over and over again that (for example) something more than $\frac{9}{10}$ths of the whole energy shall be in one-half of the vessel, and less than $\frac{1}{10}$th of the whole energy in the other half. But if the number of molecules be very great, this will happen enormously less frequently than that something more than $\frac{6}{10}$ths shall be in one-half, and something less than $\frac{4}{10}$ths in the other. Taking as unit of time the average interval of free motion between consecutive collisions, it is easily seen that the probability of there being something more than any stated percentage of excess above the half of the energy in one-half of the vessel during the unit of time, from a stated instant, is smaller the greater the dimensions of the vessel and the greater the stated percentage. It is a strange but nevertheless a true conception of the old well-known law of the conduction of heat, to say that it is very improbable that in the course of 1000 years one-half of the bar of iron shall of itself become warmer by a degree than the other half; and that the probability of this happening before 1,000,000 years pass is 1000 times as great as that it will happen in the course of 1000 years, and that it certainly will happen in the course of some very long time. But let it be remembered that we have supposed the bar to be covered with an impermeable varnish. Do away with this impossible ideal, and believe the number of molecules in the universe to be infinite; then we may say one-half of the bar will never become warmer than the other, except by the agency of external sources of heat or cold. This one instance suffices to explain the philosophy of the foundation on which the theory of the dissipation of energy rests.

Take however another case, in which the probability may be

readily calculated. Let a hermetically sealed glass jar of air contain 2,000,000,000,000 molecules of oxygen, and 8,000,000,000,000 molecules of nitrogen. If examined any time in the infinitely distant future, what is the number of chances against one that all the molecules of oxygen and none of nitrogen shall be found in one stated part of the vessel equal in volume to $\frac{1}{5}$th of the whole? The number expressing the answer in the Arabic notation has about 2,173,220,000,000 of places of whole numbers. On the other hand, the chance against their being exactly $\frac{2}{10}$ths of the whole number of particles of nitrogen, and at the same time exactly $\frac{2}{10}$ths of the whole number of particles of oxygen in the first specified part of the vessel, is only 4021×10^9 to 1.

APPENDIX.

Calculation of probability respecting Diffusion of Gases.

For simplicity, I suppose the sphere of action of each molecule to be infinitely small in comparison with its average distance from its nearest neighbour; thus, the sum of the volumes of the spheres of action of all the molecules will be infinitely small in proportion to the whole volume of the containing vessel. For brevity, space external to the sphere of action of every molecule will be called free space: and a molecule will be said to be in free space at any time when its sphere of action is wholly in free space; that is to say, when its sphere of action does not overlap the sphere of action of any other molecule. Let A, B, denote any two particular portions of the whole containing vessel, and let a, b, be the volumes of those portions. The chance that at any instant one individual molecule of whichever gas shall be in A is $\frac{a}{a+b}$, however many or few other molecules there may be in A at the same time; because its chances of being in any specified portions of free space are proportional to their volumes; and, according to our supposition, even if all the other molecules were in A, the volume of free space in it would not be sensibly diminished by their presence. The chance that of n molecules in the whole

space there shall be i stated individuals in A, and that the other $n-i$ molecules shall be at the same time in B, is

$$\left(\frac{a}{a+b}\right)^i \left(\frac{b}{a+b}\right)^{n-i}, \text{ or } \frac{a^i b^{n-i}}{(a+b)^n}.$$

Hence the probability of the number of molecules in A being exactly i, and in B exactly $n-i$, irrespectively of individuals, is a fraction having for denominator $(a+b)^n$, and for numerator the term involving $a^i b^{n-i}$ in the expansion of this binomial; that is to say, it is—

$$\frac{n(n-1)\ldots\ldots(n-i+1)}{1.2 \quad \ldots\ldots \quad i} \left(\frac{a}{a+b}\right)^i \left(\frac{b}{a+b}\right)^{n-i}.$$

If we call this T_i, we have

$$T_{i+1} = \frac{n-i}{i+1}\,\frac{a}{b}\,T_i.$$

Hence T_i is the greatest term if i is the smallest integer, which makes

$$\frac{n-i}{i+1} < \frac{b}{a};$$

this is to say, if i is the smallest integer which exceeds

$$n\frac{a}{a+b} - \frac{b}{a+b}.$$

Hence if a and b are commensurable, the greatest term is that for which

$$i = n\frac{a}{a+b}.$$

To apply these results to the cases considered in the preceding article, put in the first place

$$n = 2 \times 10^{12},$$

this being the number of particles of oxygen; and let $i = n$. Thus, for the probability that all the particles of oxygen shall be in A, we find

$$\left(\frac{a}{a+b}\right)^{8 \times 10^{12}}.$$

Similarly, for the probability that all the particles of nitrogen are in the space *B*, we find

$$\left(\frac{b}{a+b}\right)^{2 \times 10^{12}} .$$

Hence the probability that all the oxygen is in A and all the nitrogen in B is

$$\left(\frac{a}{a+b}\right)^{2 \times 10^{12}} \times \left(\frac{b}{a+b}\right)^{8 \times 10^{12}} .$$

Now by hypothesis

$$\frac{a}{a+b} = \frac{2}{10},$$

and therefore

$$\frac{b}{a+b} = \frac{8}{10};$$

hence the required probability is

$$\frac{2^{26 \times 10^{12}}}{10^{10^{13}}} .$$

Call this $\frac{1}{\mathrm{N}}$, and let log denote common logarithm. We have $\log \mathrm{N} = 10^{13} - 26 \times 10^{12} \times \log 2 = (10 - 26 \log 2) \times 10^{12} = 2173220 \times 10^{6}$. This is equivalent to the result stated in the text above. The logarithm of so great a number, unless given to more than thirteen significant places, cannot indicate more than the number of places of whole numbers in answer to the proposed question, expressed according to the Arabic notation.

The calculation of T_i, when i and $n-i$ are very large numbers, is practicable by Stirling's theorem, according to which we have approximately

$$1.2.\cdots\cdot i = i^{i+\frac{1}{2}} \epsilon^{-i} \sqrt{2\pi} ,$$

and therefore

$$\frac{n(n-1)\ldots\ldots(n-i+1)}{1.2 \quad \ldots\ldots \quad i} = \frac{n^{n+\frac{1}{2}}}{\sqrt{2\pi}\, i^{i+\frac{1}{2}}(n-i)^{n-i+\frac{1}{2}}} .$$

Hence for the case

$$i = n\,\frac{a}{a+b},$$

which, according to the preceding formulæ, gives T_i its greatest value, we have

$$T_i = \frac{1}{\sqrt{2\pi nef}},$$

where

$$e = \frac{a}{a+b} \text{ and } f = \frac{b}{a+b}.$$

Thus, for example, let $n = 2 \times 10^{12}$;

$$e = \cdot 2,\ f = \cdot 8,$$

we have

$$T_i = \frac{1}{800000\sqrt{\pi}} = \frac{1}{1418000}.$$

This expresses the chance of there being 4×10^{11} molecules of oxygen in A, and 16×10^{11} in B. Just half this fraction expresses the probability that the molecules of nitrogen are distributed in exactly the same proportion between A and B, because the number of molecules of nitrogen is four times greater than of oxygen.

If n denote the molecules of one gas, and n' that of the molecules of another, the probability that each shall be distributed between A and B in the exact proportion of the volume, is

$$\frac{1}{2\pi ef\sqrt{nn'}}.$$

The value for the supposed case of oxygen and nitrogen is

$$\frac{1}{2\pi \times \cdot 16 \times 4 \times 10^{12}} = \frac{1}{4021 \times 10^{9}},$$

which is the result stated at the conclusion of the text above.

10. On the Stresses due to Compound Strains. By Prof. C. Niven. Communicated by Prof. Tait.

(*Abstract.*)

In the treatment of questions which relate to the equilibrium vibrations of elastic solids, it is usual to suppose the substance to start from a state without strain, and in general to consider only the case of small distortions, for which squares and products of the space-variations of displacement may be neglected. The mathematical solution depends, in the first instance, on the expression of the work done in distorting an element. This, as was first done by Green, is expressed in terms of six functions of the distortions, termed strains. The part of the potential function which is of the second degree contains 21 coefficients, reducible to 18 by a proper choice of axes. But in the present state of our knowledge of the constitution of æolotropic substances, it is in general impossible to effect a further reduction, and it is probable that the function will have different forms according to the previous history of the substance.

The case in which the æolotropy has been produced by the action of considerable stress has formed the subject of investigations by Cauchy, St Venant, and others. Cauchy's results were based directly on the consideration of molecular attractions; and though the other authors have employed Green's theory of the potential energy, they have still made use of molecules to find its form.

In the present paper the author has sought to base the treatment of the subject on the law of superposition of one set of strains on another. If these states of strain be called respectively primary and secondary, it is shown that the total strains differ from the primary by linear functions of the secondary, and this whether the latter be small or large. The true form of the potential in terms of the secondary strains is thereafter readily found. It agrees so far with the result of M. Boussinesq, and furnishes expressions for the stresses agreeing to a certain extent with those originally given by Cauchy.

There is one part of the potential energy due to the secondary strains which has not hitherto been discussed. It consists of terms

due to parts in the primary potential, which are respectively of the degrees 2, 3, 4 ... in the strains. The first of these has been completely investigated in the present paper, and the potential is shown to depend on two invariants which are functions of the secondary strains, and of six quantities called the primary quasi-strains. In fact, borrowing a term from the theory of reciprocal surfaces, we may say shortly that the part of the potential energy under consideration is $\frac{1}{2}\left(\frac{m\,J_1^2}{4} + n\,J_2\right)$, where J_1 is the invariant of the first order of the secondary strains and primary quasi-strains, and $-J_2$ is the corresponding invariant for the reciprocals of these systems.

It is also shown in the present paper that these quasi-strains play an important part in the elasticity of isotropic solids; for besides the above result, it appears that, with the limitation of the potential already mentioned, the products of the stresses into the strained element-volume are directly expressible in terms of them, and that the principal axes of stress coincide with those of quasi-strains. The present paper also contains the equations which express the small motions of a strained solid, with the view of testing whether they present any analogy with the luminous waves. The results are negative, as was to be expected, there being in glass three real waves for every direction of the wave-front, and the wave surface being of the sixth class. In the case where the primary stress is symmetrical round an axis, an ellipsoid of revolution detaches itself from the general surface.

Among other subsidiary results of this paper may be mentioned the derivation (from the law of superposition of strains) of the symbolical expressions for the stresses in terms of the strain-variations of the potential energy (already found in another shape by M. Boussinesq), and the symbolized solution of the converse problem.

The law of resolution of strains and quasi-strains has been shown to be identical with that of stresses and with various other mathematical magnitudes, among which may be mentioned the system consisting of the moments and negative products of inertia of a solid body. A general view of this law of resolution of stresses is given and coupled with a parallel view of forces, along with a

general method of deducing the corresponding invariants and the derived stress- and force-functions. It enables us to see at a glance the meaning of the form found for the potential energy in the secondary strain. The author may be allowed to add, that these methods have been since developed with the view of applying them to the problem of the elasticity of crystals, and that the results obtained, though dual in form, exhibit a striking coincidence with these now given.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VIII. 1873–74. No. 89.

Ninety-First Session.

Monday, 2d March 1874.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Parallel Roads of Glen Roy. By the Rev. Thomas Brown, F.R.S.E.

(*Abstract.*)

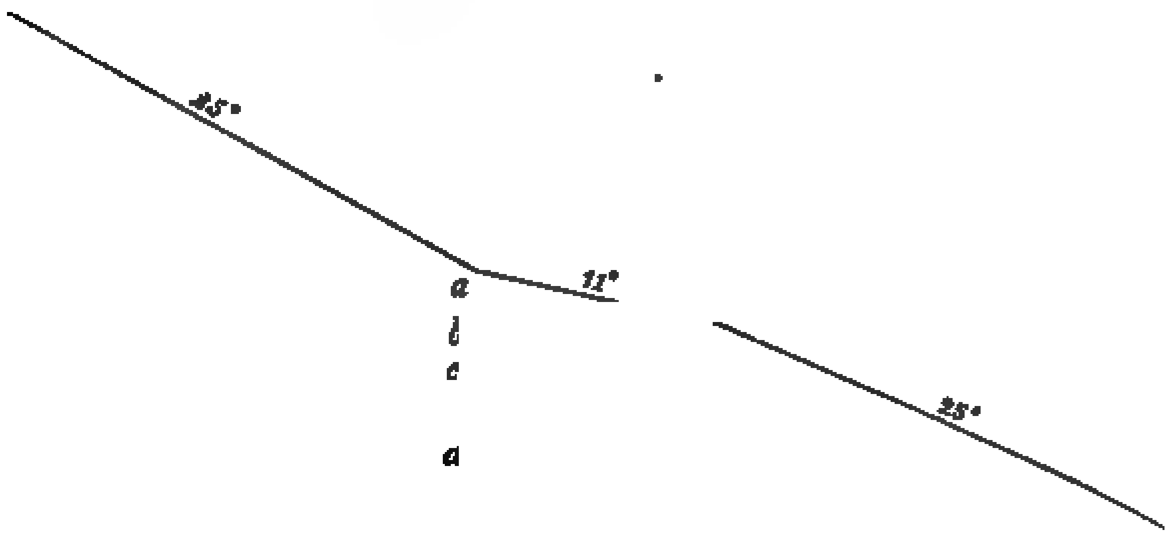
After describing the general appearance of these terraces, the author referred to the discussions which had taken place as to their formation. More than fifty years ago it was conclusively proved by Dr Macculloch* and Sir Thomas Dick Lauder,† that these parallel roads are the margins of ancient lakes, and since then the question has been whether these were freshwater or sea lochs. Mr Darwin, Dr Robert Chambers, Professor Nichol, and others, have contended that they were marine; while Agassiz, Dr Milne Home, Mr Jamieson, and others, have maintained that they were freshwater.

As the problem is confessedly one of some difficulty, it seemed desirable to obtain, if possible, the evidence of fossils. It has been ascertained, indeed, that the deposits contain no shells nor similar remains, and Mr Darwin has suggested that they may have been destroyed by the carbonic acid gas absorbed by the rain-water which for ages has percolated through the beds. This, however, could not have taken place with the valves of diatoms, which are siliceous, and

* Geol. Trans., ser. 1, vol. iv. † Edin. Roy. Soc. Trans., vol. ix.

on which, therefore, carbonic acid could produce no effect. They have this further advantage, that the marine and freshwater species each keep to their own distinct localities, and if any such fossils, therefore, could be found in the parallel roads, they might give important assistance in deciding between the marine and freshwater theories.

Accordingly, in the autumn of 1872, and again in 1873, an attempt was made to obtain fossil diatoms from these deposits. It was found that in Glen Roy there were a good many points where the parallel roads were cut through and laid open. Four of these



a, Humus—peaty, . . .	8 in. to 1 foot.	
b, Stones with clay, . . .	2 to 3 feet.	
c, Finely stratified sand and clay,	3 in. to 8 in.	
d, Clay with boulders indistinctly stratified, with thin irregular courses of sand,* . . .	about 20 feet going down to the rock.	

sections were examined with some care, and one was especially fixed on lying high and dry on the hill side, where the internal structure of the lowest terrace was distinctly shown. The object was to obtain specimens of diatoms which may have been alive when the terraces were formed, and which were then included in the deposits; but to secure this, various precautions were necessary. The nature of the different beds composing these terraces will be

* It may be a question whether this bed *d* really belongs to the time of the lowest shelf, or whether it is not some anterior formation—the sloping bottom of the lake, perhaps, at some earlier time.

understood from the preceding diagram representing the section just referred to.

In searching for diatoms, it was necessary to avoid the bed *a*, which has been formed since the time of the parallel roads. It was thought safer also to throw out of view the bed *b*, the upper surface of which is in contact with *a*. Attention was therefore confined to the beds *c* and *d*. The outside weathered portion of the bed was in each case removed, and part of the internal contents of the bed cut cleanly out—that from *d* being about 10 feet below the surface. The material thus obtained was washed in distilled water and microscopically examined. It was found that the search required much patience. Diatoms were ascertained to be present scattered very rarely through the material, but at last a series of specimens were got. These were sent to Professor Dickie, of Aberdeen, one of our highest authorities in this department of natural history, and the following species were determined by him:—

Pinnularia viridis.	Diatoma vulgare.
Himantidium undulatum.	Surirella panduriformis?

Of these the first three were got from bed *c*, and the first two and the last from bed *d*.

Now these are all freshwater species, and their evidence is strengthened by the fact that there is the entire absence of any marine diatom or other organism. This would indicate that it was an old freshwater lake which had these parallel roads for its margins. Freshwater diatoms might, indeed, have been brought down into it even if it had been a sea loch, but the important fact is, that while freshwater species are found, it has been impossible to detect a single trace of anything marine.

It is indeed true that it is only a single locality which has been searched in this way, and it would be going too far to hold the results as at once conclusive. Enough, however, has been done to show that this method of approaching the solution of the problem deserves to be followed out. Search should be made at other points along these parallel roads where they are laid open. They have been a kind of battle-field fought over by rival theorists for the last fifty years, and it will be strange if all the time multitudes

of witnesses have been lying shut up in the deposits, only waiting to be called into court to give decisive evidence. So far as the investigation has gone, it is in favour of the Freshwater Theory.

2. Note on the Perception of Musical Sounds. By John G. M'Kendrick, M.D.

Certain individuals appear to be incapable of appreciating musical sounds. They cannot distinguish one melody from another; and if by many repetitions of the melody in their hearing, they at last appear to know it, the addition of one or more of the parts of the harmony again renders the music unrecognisable to them. The question naturally arises, Is this defect owing to any peculiarity in the structure of the internal ear of persons so constituted which prevents them hearing certain sounds, or is it to be referred to the condition of the brain? On the other hand, many have what is termed a "fine ear," by which we understand the faculty of appreciating, remembering, and, in some cases, of successfully imitating musical sounds. Have those individuals the organ of hearing more delicately developed?

This physiological problem does not, in the present state of our knowledge of the minute structure of the organ of hearing in man, permit of being examined histologically. We would not probably find any appreciable histological difference between the internal structure of the ear of a genius in music and that of a person who could not distinguish one melody from another. So far as this method of inquiry is concerned, differences may exist, but the minute size of the ultimate recipients of sound-waves, and the vagueness of our present knowledge of the number of these in the depths of the cochlea, would prevent any one from noticing those differences.

It, therefore, occurred to me to examine this question by testing experimentally whether those individuals who profess to be unable to know music were incapable of hearing certain musical sounds, limited as regards pitch, within the extreme keys on the key-board of a piano. I have examined ten cases of this kind.

In a musical sound three elements have to be considered,—1*st*, loudness or intensity, which depends on the extent of vibration;

2*d*, pitch, determined by rate of vibration; and, 3*d*, quality, which depends on the orders, numbers, and relative intensities of the simple tones into which it can be resolved.

Up to the present point of this inquiry I have devoted attention chiefly to the element of quality. The apparatus I have employed was made by Georg Appunn of Hanau. It is a long wooden box inclosing a row of vibrating tongues or free reeds, which can be thrown into action by propelling air into the box by means of a bellows. The note produced by the longest reed, No. 1, is that obtained by a vibrating cord, of a certain length, thickness, and tension, as in a monochord, and corresponds to C^2, having 32 vibrations per second. On dividing the cord into 2, 3, 4, 5, 6, &c. equal segments, each segment, when caused to vibrate, will produce a note composed of 2, 3, 4, 5, or 6 times the number of vibrations in No. 1. This apparatus is capable of producing 64 tones, a larger number than are included within the key-board of a piano. The names and number of vibrations per second of these tones in this apparatus is as follows:—

No.				No.		
1.	C^2	32,	Fundamental tone.	21.	f^2	672
2.	C^1	64,	Octave.	22.	F^2+	704
3.	G^1	96,	Fifth above No. 2.	23.	Fis^2	736
4.	C^0	128,	Fourth above No. 3.	24.	G^2	768
5.	e^0	160,	Major third above No. 4.	25.	gis^2-	800
				26.	a^2-	832
6.	G^0	192,	Minor third above No. 5.	27.	a^2	864
				28.	b^2	896
7.	C^0	224		29.	Ais^2	928
8.	C^1	256		30.	h^2	960
9.	D^1	288		31.	H^2	992
10.	e^1	320		32.	C^3	1024
11.	F^1+	352		33.	C^3+	1056
12.	G^1	384		34.	Des^3+	1088
13.	a^1-	416		35.	d^3-	1120
14.	b^1	448		36.	D^3	1152
15.	h^1	480		37.	D^3+	1184
16.	C^2	512		38.	Es^3-	1216
17.	Des^2	544		39.	e^3-	1248
18.	D^2	579		40.	e^3	1280
19.	Es^2-	608		41.	E^3+	1312
20.	e^2	640		42.	f^3	1344

No.		
43.	f^3+	1376
44.	F^3+	1408
45.	Fis^3	1440
46.	Fis^3+	1472
47.	g^3	1504
48.	G^3	1536
49.	As^3-	1568
50.	Gis^3-	1600
51.	As^3	1632
52.	a^3-	1664
53.	a^3	1696
54.	A^3	1728
55.	A^3+	1760
56.	b^3	1792
57.	B^3	1824
58.	Ais^3	1856
59.	Ais^3+	1888
60.	h^3	1920
61.	H^3+	1952
62.	H^3++	1984
63.	c^4-	2016
64.	C^4	2038

I have also a series of 64 resonators, tuned to these 64 tones, and having corresponding numbers. When tone No. 12 on the overtone apparatus is sounded, and the narrow end of resonator No. 12 is placed in the ear, the instrument sings into the ear of the observer with great intensity. I have thus in the group of resonators an apparatus for analysing any compound musical note into its constituent tones; and, in the overtone apparatus, I have a means of checking the sensation of the listener by sounding, with much greater intensity, the tone corresponding to the resonator by which he heard any particular tone in a compound note. The method I adopted was,—1*st*, to strike a note on the piano, which, of course, consisted of a fundamental tone, and of certain overtones; 2*d*, to allow the person whose ear was being examined to listen with the various resonators until he selected one by which he heard a tone (one existing in the note, and strengthened in intensity by the resonator); 3*d*, after the listener had satisfied himself that he clearly heard the overtone ringing in his ear, the note on the piano was arrested, the stop of the overtone apparatus corresponding to the overtone was withdrawn, so as to sound the overtone, and the listener had to decide whether or not this was the same sound as the one he heard when listening to the musical note. The result in the ten cases I examined was as follows:—In nine of the cases the overtone was readily perceived; and in the tenth, the lower overtones of the series were observed directly, whereas the higher overtones were not noticed by means of the resonator, but were clearly observed when those overtones were sounded on the overtone apparatus. This individual asserted he had often noticed he

was deaf to very highly-pitched sounds which other people said they heard.

These results indicate that, so far as the structure of the ear is concerned, those individuals who are said not to know one note from another, are equally capable, by the use of resonators, of analysing a compound musical note—that is, of hearing the various tones of which it is composed—with those who have a good ear. Physiologically, they seem to be capable of splitting up, unconsciously, the compound vibration into the simple vibrations, the rates of which are once, twice, or thrice that of the fundamental note.

The next point which I examined was regarding the perception by persons having no musical ear of difference and summation tones, which, as is well known, play an important part in the theory of concord and discord.

If, on the overtone apparatus, two tones of different pitch are sounded, a third and deeper tone may be frequently observed. These tones were first discovered by a German organist, Andreas Sorge, in 1740. For example, if 2 : 3, or 3 : 4, or 6 : 7, or 7 : 8, &c., are sounded, a third and deeper tone may be perceived by the use of a proper resonator, which will be always found to be $C_2 = 1$; that is, this combination tone is produced by 32 vibrations per second, the difference between the respective vibration numbers of the tones 2 : 3, or 3 : 4, or 6 : 7, &c. I have found that the difference tone heard with greatest distinctness corresponds to one produced by 128 vibrations per second. For example, on sounding 16 : 20, or 24 or 28, or 32 and 36, with resonator No. 4, I can distinctly hear the tone corresponding to No. 4 = 128 vibrations per second in each case. I have found no marked difference between non-musical and musical individuals in the perception of difference tones, except as regards intensity. I had the opportunity of examining two persons of marked musical ability, who could distinguish, by great attention, without the aid of resonators, difference tones to the 6th of the series, and who could observe difference tones, 2, 3, and 4, with comparative ease. Non-musical persons did not observe these difference tones without the use of resonators to add to their intensity; and, in one case, the person could not hear them at all. In addition to these primary difference tones, I have

met with only one individual who could hear what are termed secondary and tertiary difference tones, and he could not hear these without apparently a strong effort of attention. They were as follows:—On sounding 16, $C^2 = 512$ vibrations per second, and 20, $e^2 = 640$, he heard $C^0 = 128$, that is, $20 - 16 = 4$. By using resonator No. 12, he heard $12 = 384$, that is $16 - 4 = 12$; and on using resonator No. 8, he heard very feebly 8, that is $12 - 4 = 8$. When C^2 and e^2, that is tones corresponding to 512 and 640 vibrations per second, were sounded in this person's ears, he heard other three tones with the use of resonators, namely, those produced by 128, 256, and 384 vibrations per second.

But when two tones are sounded, in addition to a tone produced by a vibration number equal to the difference between the vibration number of the two, another tone is produced, the vibration number of which is equal to the sum of the vibration numbers of the two primaries. This tone is called a summation tone. For example, on sounding $4 = 128$ and $6 = 192$ vibrations per second, by using resonator No. $10 =$ a tone having 320 vibrations per second may be distinctly heard. Thus, 2 : 3, and 3 : 4, and 5 : 9, will produce sounds heard by resonators Nos. 5, 7, and 14, respectively. I have found that non-musical people can hear these summation tones with great distinctness if increased by resonators. They can hear the lower order of summation tones much more easily than the higher order. For example, all could hear the summation tone 2 (64) : 3 (96) = 5 (160) –, or 4 (128) : 6 (192) = 10 (320); but only four out of the ten could hear 7 (224) + 8 (256) = 15 (480), and 8 (256) + 9 (288) = 17 (544). Only one out of the ten could hear 30 (960) + 28 (896) = 58 (1856), and none could hear 32 (1024) + 30 (960) = 62 (1984). I observed also that they could hear the higher summation tones only when the intensity was increased to as great an extent as possible. The two musical persons examined were able to hear all these sounds with ease, with even diminished intensity.

According to Helmholtz, there are secondary and tertiary summation tones, which spring from combinations of the primary summation tones with its elements. Thus, 3 (96) : 5 (160), with the overtone apparatus, give 8 (256); 3 (96) and 8 (256) give 11 (352); 5 (160) and 8 (256) and give 13 (416). Therefore, when 3 (96) and 5 (160) are sounded, according to this statement, the listener with

resonators may hear 3 (96), 5 (160), 8 (256), 11 (352), and 14 (448). I have examined this and various other combinations. I can with my own ears hear, by using the appropriate resonator, the primary combination series quite distinctly, but no farther. The secondary tones I have never heard. Eight out of the ten non-musical people I have examined have heard the primary series; the other two said they thought they could hear the second. The two musical persons asserted they could hear the tones distinctly.

If then "the presence of overtones confers on music its most characteristic charms," as stated by Sedley Taylor,* it appears to me that non-musical persons, when aided by resonators, are as capable as musical persons of recognising the existence of certain of these overtones. The difference between the two classes of listeners is either—(1), that the intensity of the overtone requires to be greater to be appreciated by a non-musical than by a musical person; or (2), that musical persons, by previous education of the sense, are better able to appreciate distinctions of sound. Non-musical persons seem to be incapable of noticing the existence of the higher overtones, which are, of course, much less intense than the lower overtones. They are incapable of observing the difference and summation tones having high vibration numbers. Thus, so far as the mere perception of musical sounds, and of those secondary vibrations, which produce overtones, and give quality to the fundamental tone, or duad, or triad, &c., is concerned, non-musical persons are affected by the vibrations just as musical persons are affected. The only difference I have noticed between the two is that of intensity. A musical person hears tones of low intensity, such as the higher overtones, quickly, and apparently without difficulty; whereas, a person who is non-musical hears the lower overtones, but he cannot hear the upper at all, even with the aid of a resonator. The question of intensity of tones and overtones I have still under experimental inquiry. These researches indicate that in the sense of hearing there is no state analogous to that of colour-blindness in the eye.

* Sound and Music: A Non-Mathematical Treatise on the Physical Constitution of Musical Sounds and Harmony, &c. By Sedley Taylor, M.A., &c. London, 1873.

3. On the Establishment of the Elementary Principles of Quaternions on an Analytical Basis. By G. Plarr, Esq. Communicated by Professor Tait.

4. Preliminary Note "On a New Method of obtaining very perfect Vacua." By Professor P. G. Tait and Mr James Dewar.

Professor Andrews, in the "Philosophical Magazine" for 1852, recalled the attention of physicists to the method originally devised by Davy of making a vacuum so perfect, that the residual gas exercised no appreciable pressure as registered by the depression of a barometric column. This he effected by filling the vessel to be exhausted with carbonic acid gas, having previously inserted a cup containing a concentrated solution of caustic potash. On rapidly exhausting with an air-pump, and leaving time for the absorption of the residual carbonic acid by the caustic potash, he obtained a vacuum as perfect as a Torricellian. Andrews' method was afterwards employed by Gassiot in his well-known investigations on the passage of electricity through attenuated media. By the use of stick potash in carbonic acid vacuum tubes, he succeeded in rendering the tubes so free from any traces of gas, that the electric discharge will not pass. Caustic potash for this purpose is unsatisfactory, from the fact of its requiring to be fused before rapid absorption takes place, and also from the fact that aqueous vapour is apt to be left. This plan is besides entirely confined to carbonic acid tubes, although other chemical agents might be procured to effect absorption of traces of other gases. The method we have devised to absorb traces of gases is based on the remarkable power of absorption of cocoa-nut charcoal for gaseous bodies generally. By placing a piece of this charcoal in a glass tube, having two platinum terminals for the purpose of passing an electric discharge and exhausting with a Sprengel pump, heating the charcoal to a low red heat during the exhaustion, when the tube is sealed the vacuum is so perfect that no spark will pass with a coil giving quarter of an inch sparks in air. On now heating the charcoal with a spirit lamp, sufficient gas is given out to allow the spark to pass; on cool-

ing, rapid reabsorption takes place, and the tube is rendered again impervious to the discharge. This operation may apparently be repeated *ad infinitum* with the same results.

Many important determinations may be effected through the employment of carbon vacua, such as the temperature at which dissociation takes place between the carbon and the dissolved gas, the time required for reabsorption, and the effect of different gases in influencing the action of the carbon. We need hardly say that this easy means of obtaining vacua will be of importance in spectroscopic observations, and we intend shortly to communicate observations in this direction.

5. Laboratory Notes. By Professor Tait.

1. *On Atmospheric Electricity.*

For some days past I have been in the habit of observing atmospheric electricity about one o'clock, with the view of ascertaining whether the concussion produced by the time-gun has, as I suspected from an experiment of ten years ago, an effect on the amount collected by the water dropper. For several successive days the atmospheric charge was small and only slowly variable, and uniformly on these occasions I found a sudden slight increase of the deflection of the electrometer (whether it was originally positive or negative) to occur simultaneously with the sound, precisely the result obtained in the single experiment of date 21st May 1864. It appears to be most probably due to mere shaking of the instruments.

On Thursday last, the 26th, during the great storm, the amount of electricity collected was so large, as in general to be beyond the range of my divided ring electrometer after a fraction of a second. I therefore connected the water dropper with a gold leaf electroscope, whose leaves were thick, and about five inches long by one broad. These leaves are made to diverge so as to touch the tinfoil coating of the case in periods often less than a quarter of a minute, indicating a potential of, roughly speaking, many thousand Grove's cells. The most curious phenomenon, however, was this, that at intervals. often not exceeding a minute, and while rain and hail were alternating, the charge of the electroscope, even on this large

scale, changed from + to − and back again. It seemed, in fact, as if there were alternate changes of atmospheric potential from high + to high −, and not, as is usual in such weather, changes merely from higher to lower −. The water dropper projected $2\frac{1}{2}$ feet from the wall of the College at an elevation of $44\frac{1}{2}$ feet from the ground below, and discharged at an average about 2·5 cubic inches of water per minute.

2. *On the Thermo-Electric Position of Sodium.*

I owe to Mr Dewar's skill in manipulation the means of determining the line of sodium in the thermo-electric diagram. He constructed for me a long quill tube of German glass, with platinum wires inserted near the ends; exhausted it by means of a Sprengel pump, and drew in melted sodium from a bath of paraffin. Exact determinations will require considerable time, even with this excellent apparatus; but in the meantime I may state (as a first approximation) that the line of sodium is nearly parallel to that of palladium, and somewhat above it in the diagram.

The following Gentlemen were elected Fellows of the Society:—

John Anderson, M.D.
James Napier, Esq., Glasgow.
Alexander Hunter, M.D., F.R.C.S.E.

The following Gentlemen were elected Honorary Fellows, to supply the vacancies caused by the deaths of Sir John Herschel, Sir Roderick Murchison, John Stuart Mill, Hugo Von Mohl, Wilhelm Karl Haidinger, Baron Justus von Liebig, and Gustav Rose:—

1. *British Honorary.*

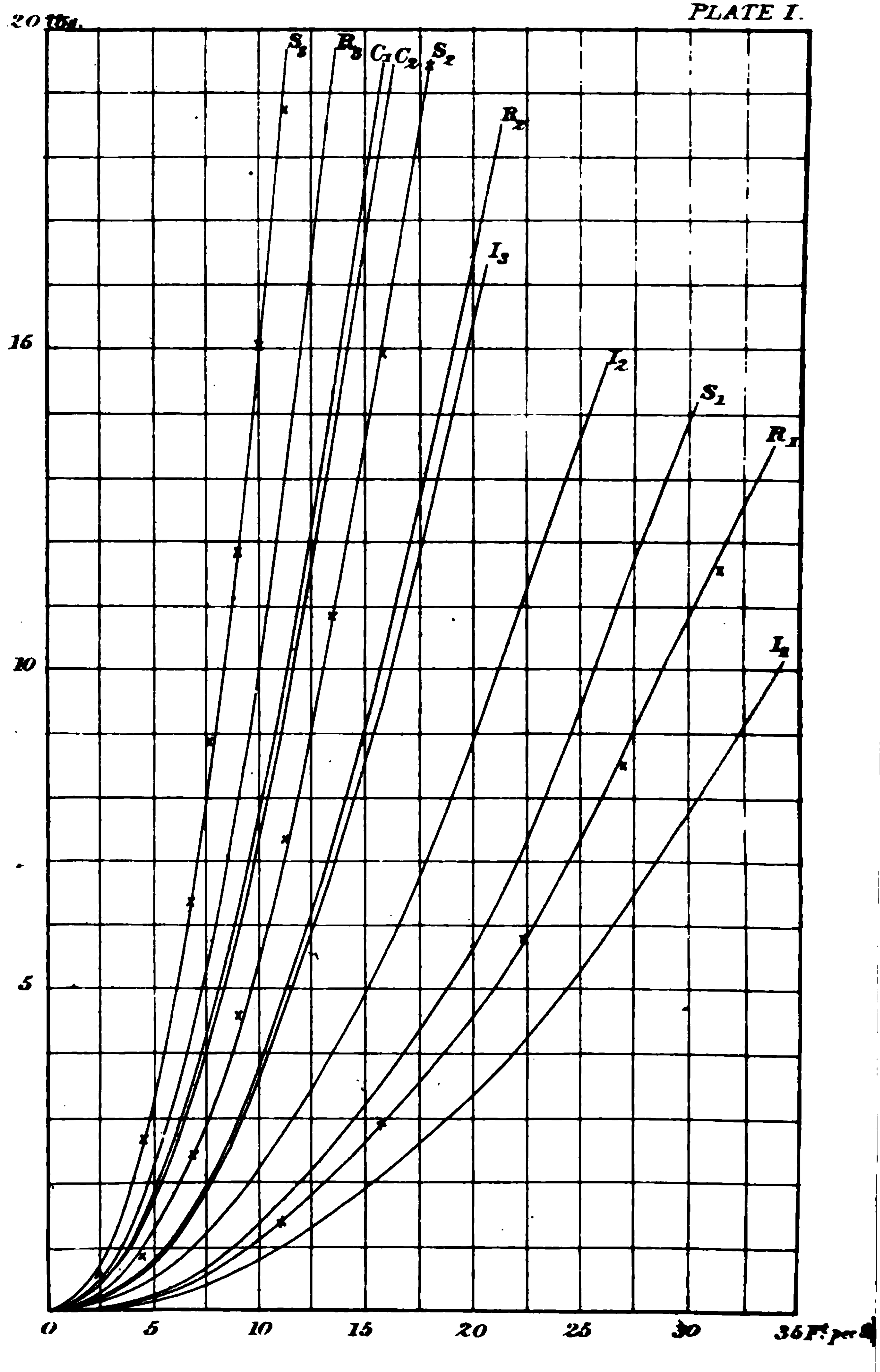
James Joseph Sylvester, LL.D., London.
William Hallowes Miller, LL.D., Professor of Mineralogy, Cambridge.
John Anthony Froude, London.

2. *Foreign Honorary.*

Adolphe Théodore Brongniart, Professor of Botany, Paris.
Louis Pasteur, Paris.
Wilhelm Eduard Weber, Gottingen.
Otto Torell, Professor of Zoology and Geology, Lund, and Director Geological Survey of Sweden.

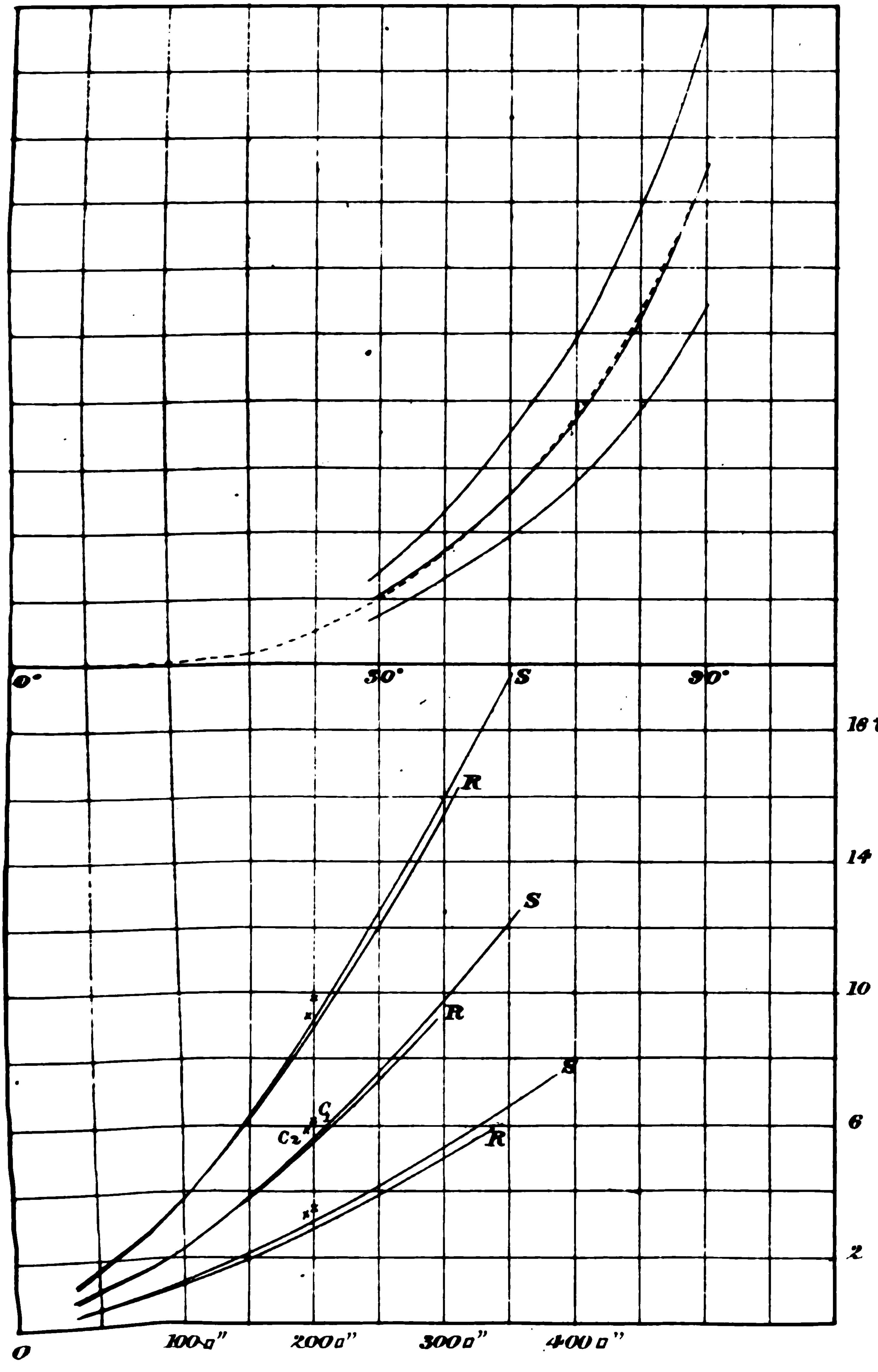
RESISTANCES IN TERMS OF VELOCITIES

PLATE I.



RESISTANCES IN TERMS OF SINES OF INCLINATIONS

PLATE II.



RESISTANCES IN TERMS OF AREAS

Monday, 16th March 1874.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Resistance of the Air to the Motion of Fans. By James C. Fairweather, Esq. Communicated by George Forbes, Esq. (With two Plates.)

The design of this paper is to describe the results of some experiments on "the Resistance of the Air," which I made under the superintendence of Professor Forbes, in the physical laboratory of the Andersonian University, Glasgow. The apparatus employed resembles somewhat that used by M. le Chevalier de Borda, whose experiments on "the Resistance of Fluids" are recorded in the "Mémoires de l'Académie Royale des Sciences." It consists essentially of a wooden frame, which was firmly secured to the floor, and supporting a horizontal axis, at one extremity of which is fixed a wooden arm, on to which were bolted the vanes of different forms and sizes, the resistance of which it was desired to ascertain. A cylinder or barrel, about 3 inches in diameter, is fastened to the axis, whereon was wound a cord, which, being acted on by weights, gave a circular motion to the axis, and consequently to the arm, thereby carrying the vanes in the circumference of a circle of 3 feet diameter, and causing them to impinge upon the air with velocities due to the force applied. The moving force consisted of weights of from ½ lb. to 20 lbs., suspended at the end of the cord, which passed over a guide-pulley, made fast in such a position that a drop of 35 feet was obtained. For each experiment the cord was wound on to the cylinder by hand, and afterwards abandoned to the action of the weight at a beat of a second's pendulum; and the vanes thereby allowed to make a definite number of revolutions, ascertained by a distinct mark on the cord; the times of which were recorded for each observation. After a few trials the pendulum

was replaced by an ordinary metronome, adjusted so as to make a vibration every half second. This instrument, from the distinctness of its beats, was found much better adapted to our purpose. The time, by this means, could be registered with perfect accuracy to one-fourth of a second of time.

The mean result of three observations was always taken with each different weight; and when there appeared to be any discrepancy, additional observations were made, in order to get a more exact average for the result. But except when the smaller weights were used this was quite unnecessary, as the results of the different observations agreed very well.

The mean result with each weight was registered, forming Table I., where, in a line with each weight, is to be found the time in seconds required for the vanes to make forty-seven revolutions.

TABLE I.

Weight on cord in lbs.	S_1 54·85 □″ Square.	R_1 41·15 □″ Round.	S_2 166·3 □″ Square.	R_2 125·8 □″ Round.	S_3 345·2 □″ Square.	R_3 264·8 □″ Round.
	″	″	″	″	″	″
1	82	73	145	140	...	...
1½	61	55	110	94	...	...
2	50	46	89	77½	138	114
3	40	36¼	70	59½	106	91
4	34	31	58	51	92	76
6	27	25	47	41½	74	61
8	23¼	21½	40½	36	64	53
10	21	19¼	37	32	56	47½
12	19	17½	34	29	52	44
14	18	16½	31½	27	48½	41
16	16¾	15½	29½	25½	45	38½
18	16	14½	28	24	43	36½
20	15	13¾	26½	22	41	34½

To ascertain the absolute resistance of the surfaces, curves similar to those on Plate I. were laid down, where the ordinates represented pressure in pounds, and the abscissæ velocities; the vanes now moving perpendicularly to their planes. In the same manner curves were laid down from Table II., which shows the weights required to give different velocities when the vanes moved in their own planes. The difference between the ordinates of a pair of curves belonging to the same vanes, the velocity being the

same in both, gives the weight which, with that velocity, was required to overcome the resistance of the air. (The skin resistance is neglected as insensible). By this means the effects of inertia and friction in the apparatus are *completely eliminated.*

The equation of the curve of absolute resistances may be put in the form—

$$R = Av + Bv^2 + Cv^3 + \ \&c.$$

When R is the resistance, v the velocity, A B and C constants. Here A and C are small, but if we include A in the expression we must have C also, for A is negative. The crosses marked on Plate I. are the calculated results A and B alone being taken; in which case the formula of course fails for small values of v.

TABLE II.

Weight on cord in lbs.	S_1	R_1	S_2	R_2	S_3	R_3
1	46″	41″	47″	60″	61″	56″
2	28½	26½	30½	32½	35	33½
3	22½	21½	24	25	28½	26½
4	19¼	18½	21½	20½	24½	23½
5	17¼	16¾	...	...	21¾	20¼
6	15¾	15¼	17	17	19¾	18¼
8	...	...	15	14¾	...	...
10	...	...	13¼	13	...	...
12	...	...	12	12	...	...

In the case of the smaller surfaces, with high velocities, the resistance would appear to increase in a somewhat greater ratio. Comparing this with the results of Dr Hutton (who gives a voluminous description of his experiments in the third volume of his "Mathematical Tracts") we find them to agree. He found that "the resistance to the same surface with different velocities, is in the case of slow motions, nearly as the square of the velocity; but, gradually increasing more and more above that proportion as the velocity increases." This is rendered obvious by calculating the index of the power after his manner, and tabulating the results as annexed.

Velocity.	Resistance.	Index.
·01	10·5	...
·015	24·5	2·089
·02	43·5	2·050
·025	70·0	2·070
·03	101·0	2·121
·035	150·0	2·122
·04	195·0	2·107
		Mean, 2·093

The index of the power of the velocity is set down in the third column for the resistance due to the curve S_2. By comparing the first velocity with each of the following ones, it will be seen that the numbers in the index column slowly and gradually increase, and would doubtless continue to do so to a very great extent. The mean of these is 2·093; whence it would appear that with these velocities, the resistance to the same surface is nearly as the 2·093 power of the velocity.

The curves marked R_1, R_2, and R_3 are derived from circular plane surfaces of 41·15, 125·8, and 264·8 square inches respectively. Those marked S_1, S_2, and S_3 are from square plane surfaces of 54·85, 116·3, and 345·2 square inches respectively. The lines C_1 and C_2 are from circular concave surfaces of 199 and 192·5 square inches, and their radii of curvature are 24·″375 and 12·25 inches respectively.

All these curves were obtained by means of vanes having their plane surfaces at right angles to their plane of rotation. The curves I_1, I_2, and I_3 were derived from a plane square surface of 166·3 square inches, inclined at angles of 30°, 45°, and 60° to the plane of rotation.

The curves on the lower part of Plate II. are intended to exhibit the manner in which the resistance increases with the surface. The abscissæ of these curves represent the areas of the surfaces in square inches, and the ordinates resistances; the velocity remaining constant. It is at once seen from these curves, that the resistance does not vary directly as the surface; but increases in a somewhat greater ratio. Within the limits of these experiments, the compound ratio of the resistance to the surface rises from 1 to 1·7.

The curves in the upper part of the same plate are intended to

show the manner in which the resistance varies with the inclination of the vanes to the plane of rotation. In these the abscissæ denote the sines of the angles of incidence (the angle of incidence being the angle which the vanes make with the plane of rotation), and the ordinates resistances.

The equation which satisfies these curves, is—

$$\mathrm{Sin}^3 i = \mathrm{R} \times \mathrm{C}$$

where i, the angle of incidence, R, the resistance, and C, a constant.

This being shown by a dotted line, found by calculation from this formula, and which almost coincides with the curve found from the experiments. This clearly proves that the resistance varies as the cube of the sine of the angle of incidence.

The curves C_1 and C_2 and the points C_1, C_2 on the curves of areas represent the effects due to concavity of the vanes; from which we conclude, that a certain amount of concavity offers a greater resistance than the same area, and configuration of a plane surface. But, on comparing the greater with the less concave surface, there appears to be little or no difference within the limits of these experiments. This appears to be due to the manner in which the particles act upon the surface. First, in comparing the concave surface with a plane surface of the same area, we find that the concave vane offers most resistance. This may be accounted for, by imagining a certain quantity of the particles to be caught in, as it were, in front of the vanes, and consequently forming a denser medium; this extra dense medium being continually kept up in front, while the vanes are in motion. This overcrowding of space has a tendency to prevent the particles from moving past the perimeter of the vanes with the same ease, and consequently retards the apparatus.

Again, by comparing the less with the more concave, we would at first sight conclude that this was simply an amplified case of the foregoing; but here we have something to balance the extra dense medium in front, viz., the action of the particles of the convex surface behind. Their action may be said to be analogous to the action of the water closing in at a ship's stern; and, therefore, tends to impel the surface forward, and in that way diminish the effects of any resistance due to the extra concavity in front. So that, looking at the matter in this light, we should conclude that

there would be a maximum resistance with a certain degree of curvature This, however, cannot be proved by the small number of observations made curved surfaces, but would be very interesting to ascertain experimentally.

2. On the Curve of Second Sines and its Variations. By Edward Sang, Esq.

The idea of this class of curves arose during an attempt to resolve an important problem in the doctrine of wheel-work; a statement of the conditions of that problem is thus the natural introduction to the subject.

When the shape of the tooth of one wheel A has been assumed, the shape of the tooth of another wheel B, to work along with it, may be deduced by a very simple graphic process; and when these two wheels are made to turn together, the points of contact describe a certain line or *path*. In my "New General Theory of the Teeth of Wheels" it is shown that this path, and the manner of motion in it, are independent of the size of the second wheel B, and result entirely from the arbitrarily assumed form of A; that is to say, if we delineate the form of a new wheel B, to work along with A, the path and the motion of the contact point along it are the same as before; the originally assumed form A thus gives rise to a system of conjugate forms B. Not only so, but if we use any one of the wheels B as an originator, we shall obtain from it a system of conjugates A, of which our first wheel A is a member. Thus the assumption of one wheel gives rise to two conjugate systems, A and B, so related that any wheel of the one works with any wheel of the other, the contact path remaining the same for every couple. It does not, however, follow that two wheels of the same set can work together; in the arrangement of wheel-work it is important that the two systems be identic. Now, when the wheel is indefinitely enlarged, its boundary merges into a straight rack, and the rack A is necessarily a copy of the rack B; hence we come to the most important theorem in the doctrine of engrainage, that "if we assume for the outline of a straight rack any curve consisting of equal undulations, symmetrically arranged on either side of its pitch-line, all the wheels determined by it work with each other,

and are reversible face to face." This theorem is gene
as geometry is concerned, but is restricted in its mechani
cation by the condition of material continuity, and he
arise some difficult and interesting problems.

Each assumed form of undulation has its peculiar pa
point of contact; this path is obtained by drawing nor
meeting the line of abscissæ in N, as in figs. 1 and 9,
through some fixed point Q, technically called the *p*
drawing QP′ equal and parallel to NP.

If we suppose the point P, in fig. 1, to move steadily
curve RXTYV, accompanied by the normal, the point N
continuously, though not uniformly, from R to V; and
point in the axis RSTUV can more than one normal be
the curve. Such a rack would give rise to a system
having only one point of contact, and so useless in m
On augmenting the ordinates HP in some fixed ratio, w
curve with deeper undulations, and augment the subnor
in the duplicate ratio; in this way we may cause the p
pass beyond S before P has arrived at X. In such cas
become stationary, and then return to S, when P shall
passing back towards R, N must again become statio
thereafter progressive, reaching T just when P does so.
way the motion of N along RV will resemble the direct
grade movements in longitude of the superior planets.
the line of abscissæ will thus be traversed thrice, once
once backward, and once forward again; and from eacl
these parts three normals may be drawn to the curve. B
adapting the ratio of enlargement we may cause the first s
position of N to be at T, and then every part of the pit
traversed thrice,—that is to say, wheels deduced from su
must always touch in three points. If we augment the
in such a ratio as to bring the first stationary position of N
to U, every part of the pitch-line will be traversed five ti
the corresponding wheels will always touch at five places.
way, when the general character or equation of the curve
mined on, we can discover the exact depth of tooth gi
specified odd number of contacts.

In machinery we should have at least two teeth completely

for which there must be seven contacts, and the first stationary position of N must be brought forward to V, as is the case in fig. 8.

The most convenient process for tracing the shapes of wheel-teeth from such a rack is to determine the positions of the contact points corresponding to equidifferent positions of the wheels, and to combine the motion along the path with the proper angular motion of a blank disc. In this way we obtain very readily the outline of the wheel; this outline, however, though always giving the proper number of contacts geometrically, is not always mechanically possible; for low-numbered wheels it is exceedingly convoluted, as is seen in fig. 9, which is that of a wheel of *one* tooth, belonging to the system of fig. 8. As the number of teeth is augmented, the convolutions become less, and at a certain limit, the limit of mechanical possibility, they disappear.

Hence arises an exceedingly important and most difficult problem, "To discover that form of rack which, giving a determinate number of contacts, shall admit of the lowest numbered wheels." The idea of the curve of second sines occurred in the attempt to resolve this problem.

Here the condition of optimism cannot be put in the form of maximum or minimum, so that the known methods of analysis are inapplicable, and we must have recourse to successive trials with known or with invented lines, and after all we can only conclude that such or such a curve is preferable to any other that has been tried.

The simplest line, consisting of an endless series of equal and symmetric undulations, is the well-known curve of sines; this curve, when used as the form for a rack, gives convolutions on the outlines of wheels of considerable size, and it becomes desirable to obtain a better shape; for this we are naturally led to try modifications of the curve of sines.

If we write u for the absciss, y for the ordinate, and v for some variable arc, the equations

$$u = v + \phi \sin 2v\,;\; y = \mathrm{A} \,.\, \theta \sin v\,,$$

in which ϕ and θ represent two unknown functions, will give equidistant and symmetric undulations, it being essential, however, for our purpose that $\phi 0 = 0$; $\theta 0 = 0$; $\phi(-2) = -\phi(+2)$ and $\theta(-2) = -\theta(+2)$. We have thus an endless variety of modifications among which to make our trials.

On omitting the term $\phi \sin 2v$, and putting $\theta \sin v = \sin \sin v$, we obtain the equation

$$y = \mathrm{A} \, . \, \sin^2 u \, ,$$

that of the curve of second sines. The transition from the ordinary wave-line to this curve is abrupt, and symbolically of the same nature as the transition from the straight line to the curve of sines itself, as is seen on comparing the three equations

$$y = \mathrm{A} \, . \, u \, ; \; y = \mathrm{A} \, . \, \sin u \, ; \; y = \mathrm{A} \, . \, \sin \sin u \, ;$$

but for the elucidation of the theory of wheel-teeth we require a gradual transition from the one kind of curve to the other; that is to say, we must obtain some comprehensive genesis which shall include both species of curve, and permit of an imperceptible change from the one to the other.

If a body vibrate along the straight line AOB, in virtue of some elastic arrangement whereof the redressing tendency is proportional to the distance from the mean point O, and if, while it is so vibrating, a sheet of paper be carried over it with a uniform velocity in a direction perpendicular to AOB, the trace made on that sheet is a curve of sines.

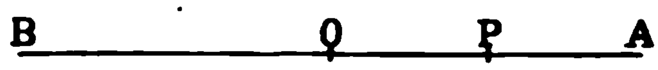


Fig. 1.

Instead of the rectilineal oscillation, let us use the motion of the balance-wheel of a watch—that is to say, let the vibration be in the circular arc AOB; and while the abscissæ, measured along the line RST, are made proportional to the times, let the ordinates be made equal to the sines *pp* of the arcs, instead of to the arcs O*p* themselves, and we shall have a variety of the curve of second sines.

If the extent of the arc be small, its sine *pp* hardly differs from itself, and the curve merges into the ordinary curve of sines. When the length of the half-arc OA is just equal to the radius CO of the circle, we obtain the curve of second sines proper, which is represented in figure 1, the base RV being equal to the circumference of the circle, whose radius is CO. As the extent of

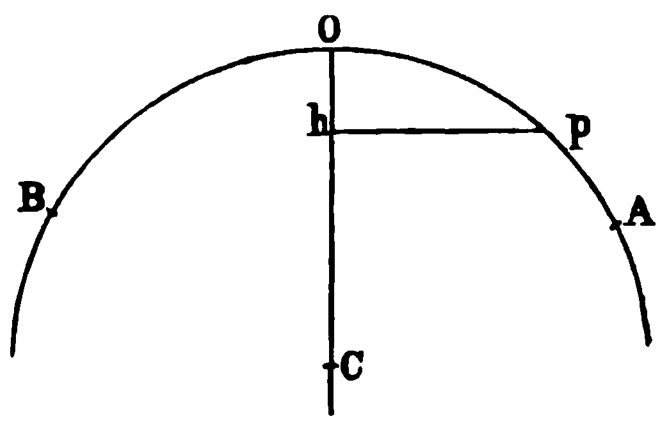


Fig. 2.

the oscillation is augmented, the curve shows its tendency to flatten at the vertex X, and when the oscillation extends over the semicumference BAO, the curve, as shown in fig. 2, becomes quite flat at X, the radius of curvature there being infinite.

Where OA extends beyond the quadrant, the ordinate rises to be equal to the radius OC, and then decreases to reach a minimum value at X, after which it again rises, and so produces the saddle form seen in fig. 3; and when the oscillation extends over a com-

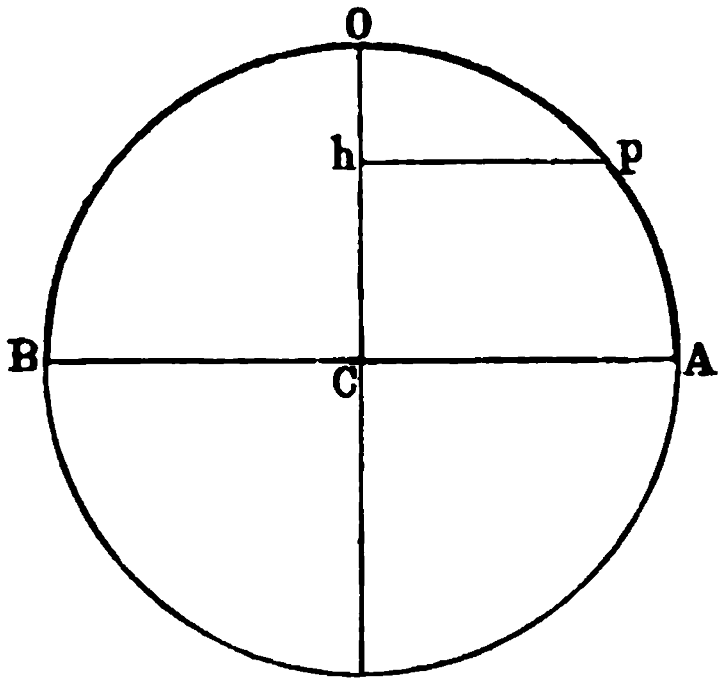


Fig. 3.

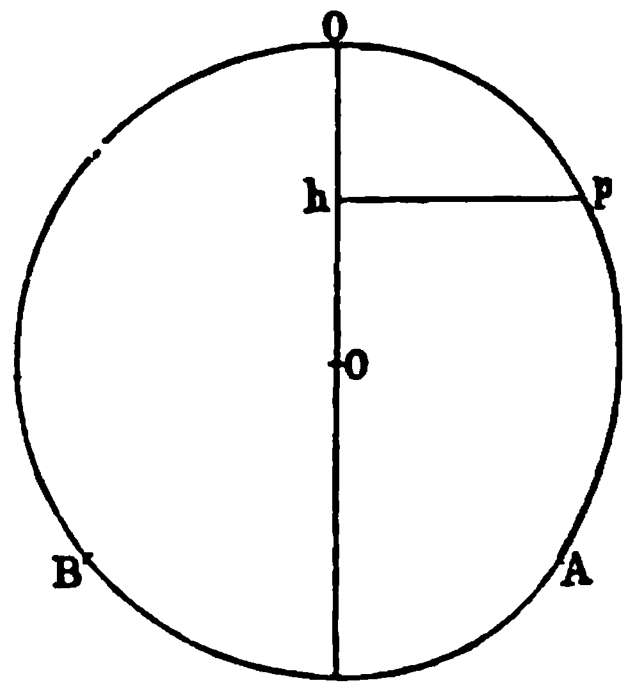


Fig. 4.

plete turn, the vertex X of the curve comes down to S, as shown in fig. 4. If the oscillation extend over more than the whole circumference, the vertex X passes to the other side of the axis, as seen in fig. 5; and when the extent is one turn and a half, the curve is again flattened on the opposite limit, preparatory as it were, to the return towards S, when the oscillation is still farther extended. Thus this genesis produces a great variety of phases, beginning with the curve of sines, passing to the curve of second sines, and continuing in an endless series of variations beyond.

As soon as we pass beyond the flattened vertex, these curves lose all interest to the practical mechanician, who can hardly contemplate the use of wheel-teeth with hollowed tops; yet to the speculative engineer they offer the attraction of peculiar phases in the configuration of the relative contact-path, and in the convolutions of the tooth outlines; but their real interest is centred in this, that amongst them we find the best known form for the rack.

When the arc OA is three-fourth parts of a quadrant, and when

the curve is raised to such a height as to have always seven normals from a point in the axis, wheels of 14 teeth, developed by its help, have their outlines mechanically complete.

Putting r for the length of OA, the half-arc of oscillation, the equation of the curve is

$$y = \text{A} \,.\, \sin (r \sin u),$$

and the length of the HN subnormal is given by the formula

$$x = \frac{1}{2} \text{A}^2 \,.\, \cos u \,.\, \sin (2r \sin u), \quad . \quad . \quad (1),$$

which gives, at the same time, the form of the contact path. Hence we have RN, the length of the pitch-line, corresponding to the contact at P,

$$\text{RN} = a = u + \frac{1}{2} r\text{A}^2 \,.\, \cos u \,.\, \sin (2r \sin u), \quad . \quad (2).$$

Hence if we denote by U that value of u which corresponds to the extreme position of the point N, we must have

$$\frac{2}{r\text{A}^2} = \sin \text{U} \,.\, \sin (2r \sin \text{U}) - 2r \,.\, \cos \text{U}^2 \,.\, \cos (2r \sin \text{U}), \quad (3),$$

and when the number of contacts is to be n, we must have the corresponding value of RN equal to $\frac{n+1}{4}\pi$; wherefore

$$\frac{\tan (2r \sin \text{U})}{\tan \text{U} \,.\, \tan (2r \sin \text{U}) - 2r \,.\, \cos \text{U}} + \text{U} = \frac{n+1}{2}\pi \; ; \qquad (4)$$

by help of which equation we can determine the values of U and A, corresponding to any assumed value of r, and to any desired number of contacts. For seven contacts, and when $r = \frac{3}{8}\pi$, we obtain A = 3·469167 and the maximum value of y, 3·205089; by help of which dimensions fig. 8 has been drawn.

If the point P be carried along the saddle-shaped curve RX of fig. 3, the subnormal HN lies first on the one and then on the other side of the ordinate PH, so that we may have two stationary positions of N as N_1 and N_2, and these may be placed so that the part $\text{N}_2\,\text{N}_1$ is traversed thrice, as actually happens in the figure. By lessening the ordinates the whole curve may be flattened, and

the problem arises, "What must be the degree of flattening in order that the points N_1 and N_2 may coincide?" in which case no more than one normal can be drawn to the curve from any point in its axis.

Again, by augmenting the ordinates, as in figure 7, the extent of the overlap N_2 N_1 may be increased, and the limiting station for one quadrant may touch that for another quadrant of the curve, and thus we may determine the character and height needed to ensure that some specified odd number of normals, neither more nor less, may be drawn from any point assumed in the axis.

In the solution of such a problem we have to consider the two roots of equation 3 below $u = \frac{1}{2}\pi$, and recurring in each successive quadrant of the curve. When r exceeds π, and is less than $\frac{3}{2}\pi$, there are three such roots, as in fig. 5, and the discussion of the number of normals becomes exceedingly involved. The consideration of the wheel systems deduced from such curves belongs to purely speculative geometry.

3. Laboratory Note. By Professor Tait.

On the Thermo-electric Positions of Sodium and Potassium.

Farther experiments with the apparatus described in the "Proceedings" of 2d March 1874, and with a similar one containing potassium, have led to the following values of the tangent of the inclination of the corresponding lines in the thermo-electric diagram. I have added its value in palladium for comparison—

Na	−·00212
K	−·00066
Pd	−·00182

To reduce these to the corresponding numerical values of the specific heat of electricity, the factor required is 4×10^{-6} of a Grove's cell.

The line of Na in the diagram intersects that of Pb at about −20° C, and the line of K intersects that of Arg about the same temperature. By the help of these data they may easily be inserted in my diagram in vol. xxvii. part i. of the Transactions R. S. E.

4. On a New Form of Mariner's Compass. By Sir William Thomson.

Monday, 6th April 1874.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Further Note on Spectra under exceedingly Small Pressures. By Professor Tait and James Dewar, Esq.

2. On the After-glow of Cooling Iron at a Dull-Red Heat. By George Forbes, Esq.

The facts to be explained were observed by Messrs Gore and Barrett, and were described by the latter gentleman in the "Philosophical Magazine" for 1873.

The experiments are performed on an iron or steel wire of no great thickness. When this is heated to an intense white heat and allowed to cool, the following facts appear at the instant it has cooled down to a dull-red heat:—

1. The wire expands for an instant, and then continues its normal contraction.

2. The glow from the wire is at the same instant seen to increase.

3. The temperature of the air round the wire is at the same instant increased.

4. The same facts are seen when the wire is in an atmosphere of hydrogen.

5. If the wire be very thin the cooling is so rapid that the effects are not observed.

6. If the iron be massive the effects are not observed.

7. If the wire be not originally heated up to an intense white heat the effects are not observed.

That iron should increase its temperature at a dull-red heat

while it is cooling from an intense white heat, and that it should not do so when cooling from a temperature a little over a dull-red heat, is a hypothesis so inconsistent with all known facts as to make it desirable to find some explanation more in accordance with known principles. Iron is a very bad conductor, and Professor Tait has shown (R. S. E. Proceedings, 1873) that the conductivity is much worse above than below a dull-red heat. Now, the cooling of such an iron wire as that used is effected so rapidly that the temperature falls through an enormous range of temperature in a few seconds. This is effected by convection and radiation from the surface. It is quite possible that the internal heat cannot be conducted outwards with sufficient rapidity to compensate this outer loss. Thus the temperature of the interior of the wire is greater than that of the exterior. At very high temperatures the rapidity of cooling is enormous. But as the cooling proceeds, the deviation from the Newtonian law of cooling is much less. Hence the cooling by radiation becomes less, and the heat which has been stored up in the interior of the wire has a tendency to show itself on the surface. At a dull-red heat the wire becomes a better conductor, and this tendency is assisted, so that about this stage the temperature throughout the wire is nearly equalised. The second experimental fact is explained by this raising of the external temperature. The third fact is explained in the same way. And it must be noticed, that a difference in temperature between the interior and exterior is the only means of explaining the rise in temperature of the external air, unless we suppose that, while cooling, the wire increases in temperature. And even then it would be difficult to understand why the effect is not produced by cooling from a temperature a little above a dull-red heat. If the wire be massive, or if a poker be used, the cooling is not rapid enough to produce the effects; apparently, because the convection currents are not nearly so strong in proportion to the surface which has to be cooled. Other causes come into play in this case, all tending to prevent the effect from being apparent. The explanation I have given shows why the effect is observed only when the wire has been originally heated to an intense white heat; for it is only then that a great difference of temperature can exist between the interior and exterior.

It only remains now to explain the first experimental fact, *i.e.*, the expansion of the wire at the critical instant. This follows from what has already been said, when we consider certain experiments made by Colonel Clarke, communicated to the Royal Society of London in 1863, and the explanation of them which was given by Professor Stokes. A hollow cylinder of iron was heated in a furnace, and plunged into water, so that half of it was buried in the water, the axis of the cylinder being vertical. After cooling, the cylinder was found to be permanently indented at the water-level, so that its diameter was there diminished. The explanation is as follows :—When plunged in water the lower part immediately contracts and cools. The upper part remains expanded. At this instant there is at the water-line a conflict between the upper, hot, expanded portion and the lower, cool, contracted portion. Now iron is much stronger when cool than when hot. Hence the cool iron has the advantage, and at the water-line the iron is at first forcibly shrunk, and afterwards cooled, and hence at that line the cylinder is contracted.

Now, exactly the same thing may happen in the cooling wire. Before cooling down to the dull-red heat, the hot inner part is expanded, and the cooler outer part contracted, and owing to the greater strength of the cooler iron, the wire is on the whole unduly contracted. But at the moment of after-glow the internal heat is driven out, and the contraction is no longer maintained. Hence the expansion at that temperature.

The hypothesis I have now given explains all the facts observed; but it cannot be stated to be proved. An alternative, and only one remains, which is to consider that *when iron is heated to an intense white heat it becomes different in its nature from cold iron, and that the iron in the hot state has a certain amount of latent heat, which is given out when, by cooling, the iron changes its nature.*

In the absence of any data for determining between these two, I prefer the former hypothesis, as it does not involve a new property of iron quite unlike that of any other substance yet examined. The apparently opposite phenomena observed when the iron is massive can be explained equally well on either hypothesis. But the second hypothesis is favoured by certain experiments made by Professor Barrett while heating the iron.

3. On a Form of Radiation Diagram. By George Forbes, Esq.

The following facts appear to have been conclusively established by universal experience:—

1. Nearly all, if not all, solid substances become self-luminous at the same temperature.

2. The red rays are the first to become visible, and, on increasing the temperature, colours of less wave-lengths are successively added in the order of their wave-lengths.

3. While colours of shorter wave-lengths are being added, those which were previously visible become more intense.

It appears, then, that the intensity of radiation (i) of any particular colour is connected with the temperature θ, and the wave-length λ, by some equation

$$i = f(\theta, \lambda).$$

No data at present exist by means of which the form of this function can be determined. Theoretically, however, its determination is of great importance, and it also leads to some practical applications. For this reason it is worth while making an attempt to approximate roughly to a radiation diagram, on which shall be drawn curves that are isothermals, the ordinate of any point indicating the intensity of the radiation of a wave-length indicated by the abscissa, at the temperature of the particular isothermal considered.

The experimental data for an exceedingly rough approximation to such a curve exist. But difficulties of several kinds are met with.

1. If we judge the intensity of radiation by the eye, as Frauenhofer did, we can only see a limited portion of the spectrum; and if we use a thermo-pile, with the face covered with lamp-black, we have no proof that all the invisible rays are as thoroughly absorbed as we know the visible rays to be. A thermo-pile covered with chalk would not absorb the luminous rays so intensely as one coated with lamp-black. But we cannot say that lamp-black does not

behave like chalk, in this respect, to some of the invisible rays. In fact, Melloni's experiments show that this is the case.

2. The second difficulty is, that in employing the eye, a source of error is introduced by the fact that a certain intensity of radiation is necessary before a light becomes visible to the eye. This intensity (i') is evidently dependent upon the wave-length (λ) of the light considered. Hence

$$i' = \varphi(\lambda).$$

The difference between i and i', or

$$f(\theta, \lambda) - \varphi(\lambda)$$

gives a third curve,

$$\mathrm{I} = \psi(\theta, \lambda),$$

which shows the apparent intensity of any colour in terms of the temperature.

In this paper I wish to pay attention to the luminous portion of the spectrum, with the object of determining the nature of the function $\varphi(\lambda)$ as much as that of $f(\theta, \lambda)$.

I shall enumerate the different experimental facts which throw some light on the forms of these two curves.

1. Mossotti has shown* from the experiments of Frauenhofer that the curve of apparent intensities (*i.e.*, the curve $\psi(\theta, \lambda)$) is, in the case of sun-light, a sinuous line, symmetrical about the mean wave-length.

2. Draper† has shown that the radiation from the parts on either side of the mean wave-length are, with visible radiations equal.

Thus we are led to conclude that i and $i - i'$ are both symmetrical about the wave-length of mean visibility; and hence $i - i'$ or $\varphi(\lambda)$ is also symmetrical about that line.

3. Dewar‡ has shown that several methods combine to prove that the temperature of the sun is about 16,000° C.

Hence the isothermal on our diagram, corresponding to 16,000° C., has a maximum value of i for the mean visible wave-length.

* Atti Scienz. Ital., 1848.

† Phil. Mag., 1872.

‡ Proceedings of the Royal Society of Edinburgh, 1871–72.

(Here we are only speaking of luminous radiations. But it is not improbable that in the scale of wave-lengths this is true in other parts. For refraction by prisms accumulates rays of different wave-lengths so much in the ultra-red part of the spectrum that no experiments exist which can settle this point.)

4. At lower temperatures the apparent maximum is nearer to the red, *i.e.*, the maximum of the curve $i - i'$ is nearer to the red.

But the curve i' is always a minimum at the yellow. Hence, at lower temperatures, the maximum of the curve $i = f(\theta, \lambda)$ passes to the region of greater wave-lengths. (It has just been stated that the curve $i = \varphi(\lambda)$ has a *minimum* at the centre of the diffraction spectrum. This is nearly certain, because we have seen that the curve is, at any rate, nearly symmetrical about this point, and it certainly increases enormously at the two limits of visibility of the spectrum.)

The only other remark I have to make on the curves $f(\theta, \lambda)$ is, that we cannot estimate the nature of the curve in the ultra-red at present. For all we know, there may be radiations of much greater wave-length than any which lamp-black, or any other substance we know of, could absorb.

As to the curve of limiting visibility, it appears, from what has already been said, that it has a minimum in the yellow; and although from Mossotti's interpretation of Frauenhofer's observations, it would seem to be a sinuous line, I do not think that the small variations there indicated could be detected accurately in judging of the relative brightness of different colours.

We can scarcely see those parts of the spectrum that lie beyond the lines A and H respectively. The question arises as to whether they are always invisible. If this were so, the curve of limiting visibility, which we have called $\varphi(\lambda)$ would, at those two points, be an ordinate of the curve.

But I do not think this is the case. So far as I can see, the limit of the spectrum depends upon the intensity of the light. Thus, Mr Glaisher, in his report to the British Association in 1863 on his balloon ascents, stated that, at great heights in the solar spectrum, he could "see H, and far beyond," when on the ground the line "H was quite the limit."

Again, I remember (although I cannot find a reference), that Sir

David Brewster increased the limits of the visible spectrum by dilating his pupil with Belladonna, so as to increase the amount of light.

From all these considerations, I believe that the diagram here given* is not a bad approximation to such a radiation diagram as I have described, data for its accurate determination being at present unobtainable. The principal Frauenhofer lines are marked below, and the numbers along the axis of abscissæ represent *thirteenth-metres*.†

I have drawn particular attention to what I have called the curve of limiting visibility, because a consideration of it affords an explanation of some curious facts which have, from time to time, been brought before the Royal Astronomical Society.

Many fellows of that Society were puzzled by the varying colours of stars, and of Jupiter particularly, when observed with different telescopes. Mr Huggins suggested that the amount of light, as depending upon the magnifying power and aperture of the object glass, might be the explanation of it. Mr Browning tested this, and found that it afforded a complete explanation. Colonel Strange corroborated these views by an independent observation.

I have tried, in a variety of ways, to produce this result experimentally, and believe that I have at length succeeded by employing gas light, and viewing it through a number of plates of the common blue glass coloured with cobalt. This thickness of the glass allows only blue and red rays to pass; the boundaries of these bright bands in the spectrum being sharply defined. When a piece of white paper, illuminated by the gas-flame, is examined with this glass, it appears to be blue, but the gas-flame itself appears to be red. This is due to no effect of fluorescence. Now, let I_r be the intensity of the red rays of the flame as seen through the glass, and I_b the intensity of the blue rays. Also let L_r be the limiting intensity for visibility in the red rays, and L_b in the blue rays. Then,

$$I_r - L_r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$I_b - L_b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

* The diagram here referred to is not reproduced. It will probably appear in a text-book on Physics now in preparation.—G. F., 1874, April 20.

† A thirteenth-metre = $10^{-13} \times 1$ metre.

are the apparent intensities of the red and blue parts. But, if $\frac{1}{n}$th only of the light from the flame is scattered from white paper; the intensities of the red and blue rays, when the paper is examined, are

$$\frac{1}{n} I_r - L_r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and

$$\frac{1}{n} I_b - L_b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Now, it is quite possible that while (1) is greater than (2), (4) should be greater than (3). Hence the light seen from the gas-flame has, on the whole, a red tinge, while that of the paper has a blue tinge, exactly as is seen to be the case.

I hope that this attempt at approximating to a better knowledge of some theoretically important facts will be of some interest to the Society, and that the meagre nature of our data will be sufficient apology for the small advance I have been able to make.

4. On the Semicircular Canals of the Internal Ear. By Professor Crum Brown.

(*Abstract.*)

The author had laid before the Society, on the 19th January, a preliminary note containing an outline of a theory of the function of the semicircular canals of the internal ear. In that note it was stated that the six semicircular canals form three pairs—the members of each pair being parallel, and having their ampullæ at opposite ends. In this paper the author communicates the results of measurements of the position of the bony canals in a large number of animals.

The only manner in which, assuming bilateral symmetry, the canals can be arranged in parallel pairs, with the ampullæ at the opposite ends, is as follows:—In each ear, one canal at right angles to the mesial plane, and the two others making equal angles with the mesial plane. Calling the canals of the one ear *a*, *b*, *c*, and those of the other ear *a′*, *b′*, *c′*; *a* and *a′*, *b* and *c′*, *c* and *b′* are the three pairs; *a* and *a′* are coplanar, *b* is parallel to *c′* and *c* to *b′*. The measure-

ments show that this is approximately the case—deviations of 10° from parallelism being rare, even when, as is often the case, the three canals of one ear are not at right angles to one another.

The methods employed in making these measurements were explained and illustrated.

[*Note by the Author.*—Since presenting to the Society, on January 19th, the Preliminary Note on the Sense of Rotation and the Function of the Semicircular Canals of the Internal Ear, I have seen abstracts of papers on the same subject by Professor Mach and by Dr Breuer. As far as I can judge from these abstracts, while Professor Mach and Dr Breuer refer the action of rotation upon the ampullary nerves to the inertia of the contents of the canals, they do not seem to have noticed the parallelism of the plane of the superior canal of the one ear to that of the posterior canal of the other, nor to have observed that approximate parallelism of these planes is essential, if the semicircular canals are the peripheral organs of the sense of rotation.]

The following Gentlemen were admitted Fellows of the Society:—

R. H. Traquair, M.D., Mus. Science and Art.
Francis Jones, Esq., Lecturer on Chemistry, Manchester.
W. F. Barrett, F.C.S., R. College of Science, Dublin.

Monday, 20th April 1874.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On Last-Place Errors in Vlacq's Table of Logarithms. By Edward Sang, Esq.

Now fifty years ago, while engaged with some heavy calculations connected with engineering work, I became impressed with the advantage of having logarithmic tables much more extensive than those in use. The trouble of the interpolations at the early part of the table, contrasted with the convenience of the small addi-

tional part from 100,000 to 108,000 printed in Hutton, gave rise to the idea of carrying the table onwards even so far as to one million. Although the bulk of such a table appears to be an objection, and the turning of so many leaves a toil, the ease to the habitual computer of finding at once the number of which he is in search is so great as far to outweigh the opposite considerations. Thus, though working only to five places, we prefer to use the extensive seven-place tables rather than to take up Lalande's small volume; and so, while working to seven places, we should gladly avail ourselves of a nine-place million table, the construction of which I proposed to myself, notwithstanding the vast amount of the labour.

The first idea was to interpolate from tables already published, but this was opposed by the feeling of dependency on the accuracy of the previous calculations. On examining the sources of our information on denary logarithms, it became apparent that the original work of Henry Briggs (1620), carried on in the laborious way indicated to him by John Nepair in his "Constructio," is the only foundation; and that the completion of the canon by Adrian Vlacq (1628) was the last of the original labour that has been bestowed on this matter so essential to the progress of exact knowledge.

The more convenient methods of calculation developed by the progress of logistics have come, as it were, too late to be of service. It is indeed surprising that, after the lapse of two hundred and fifty years, we are still relying on the unchecked calculations of Briggs and Vlacq; that among so many generations of scientific men there has not been zeal enough to effect a revision of the canon.

Even on the supposition that Vlacq's logarithms are true in the last place, the attempt to interpolate between them would lead to frequent uncertainty in the seventh place. In order to form an extensive table of seven-place logarithms true in the last figure, we should have to carry our original computations at least five steps farther.

Thus I came to perceive the necessity of making the whole computation anew. From time to time I took up the work to lay it down in alarm at its magnitude, for years of labour only seemed to make a beginning; but about 1849 I happened to obtain a copy

of the great "Table des Diviseurs," by Burckhardt. The facility afforded by this admirable work for finding convenient formulæ of approximation, determined me to persevere in the construction of the large table; and, putting aside all my previous calculations, I arranged a comprehensive scheme for recording each step of the process, so that it might serve as occasion might arise to facilitate subsequent steps, and so that any suspected error might be traced to its source. By this means the progress of the work was effectually secured, because each little addition took its proper place, at however long an interval of time it might happen to be made.

Without going into the details of the procedure, it is enough to mention here that the logarithms of prime numbers up to 3600, and of many others occurring incidentally, have been computed to twenty-eight places with the view of being exact to twenty-five, and that the logarithms of all their products under 10,000 have been tabulated; and, by help of these, tables have been made to fifteen places of the logarithms of all numbers from 100,000 to 320,000, with their first and second differences. These, filling in all twenty-four quarto volumes, are laid on the Society's table.

Henry Briggs computed to fourteen places the logarithms of all numbers up to 20,000, and of numbers from 90,000 to 100,000; so that Vlacq, in shortening them to ten places, was safe from error excepting in one or two rare cases. But when Vlacq set himself to fill in the intermediate 70,000, he sought to lessen the labour by using only twelve places, thus making his last figure insecure in many more cases; and, moreover, the process followed by him wanted the quality of self-verification. On these accounts I suspected the occurrence of last-place errors in Vlacq's part of the table. Seeing that each tenth logarithm of my own computation from 200,000 to 300,000, should agree with Vlacq's from 20,000 to 30,000, the comparison was made, and the result was the discovery of forty-two errors in this single myriad—an exceedingly small number when the nature of the process is considered, but a very large number to have escaped detection for two centuries and a half. At the same rate for each of the remaining six myriads, we may expect a total of nearly three hundred errors.

In 1658, that is thirty years after Vlacq, John Newton published a translation of Gellibrand's "Trigonometria Britannica," in which

he gives an eight-place table of logarithms arranged in the compact manner now usually adopted. In the address to the reader, he speaks contemptuously of Adrian as "Vlaq the Dutchman," "from whose corrupt and imperfect copy," &c.; and in the introduction he describes a mode of computing logarithms which the innocent reader may believe to have been followed by the author of the book, but a collation shows that Vlacq's misprints have been slavishly copied by the indignant Newton.

It was not until 1794 that anything claiming to be a revision of the original table appeared; this was the ten-place table given by Georg Vega in his "Thesaurus Logarithmorum," the arrangement being after the compact manner introduced by Newton. Vega gives a long list of corrections on Vlacq's table, which by that time had become scarce, and it was generally understood that he had at least taken the precaution of adding up Vlacq's differences in order to eliminate the misprints. But on collating the list of errors which I have just discovered in Vlacq, with Vega's table, we are forced, however reluctantly, to the conclusion that Vlacq's identical table had been used by the compositor of Vega's pages. A review of the character of the errors will make this clear; a list of them is subjoined, showing the logarithms true to fifteen places (the first five being omitted), the last group as it should have been in Vlacq, Vlacq's corresponding five, and Vega's last group of three.

Of the forty-two errors shown in Vlacq, forty are last-place errors, such as we are considering; and two, marked with asterisks, are misprints, as is known by the circumstance that the adjoining differences are correct. As was to have been expected, all the final errors are copied by Vega, who never pretended to have made a new computation; of the misprints one, a 9 for a 6, is corrected; but the other, 646 instead of 626, is retained. Not only so, among the final errors there are six belonging to numbers ending in 0; now these logarithms occur in the preceding part of the table, where they are correctly given, and yet these also, of easy detection, are retained by Vega. Thus, again, Vega is only Vlacq in a new and much more convenient form.

The only work claiming to be an original computation of logarithms is that done in the Bureau du Cadastre, at the instance of the French Government. This unpublished work contains to

nineteen places the logarithms of numbers from 1 to 10,000, and to fourteen places of those from 10,000 to 200,000. In the year 1819 the House of Commons, on the motion of Mr Davies Gilbert, presented an address to the Prince Regent, recommending that our Government should join with that of France in the expense of publishing these and the accompanying Trigonometrical Tables; but the negotiations fell through, for reasons that have not been made public. I have not learned that these computations have been used for the verification of those already printed, or that they have served for the production of any seven-place table; and thus, up to the present moment, we have no verification of Vlacq's great work.

The eminent astronomer Lalande, in publishing his little five-place table, was able confidently to assert that it does not contain a single error, and although many thousands of copies have been in use now for seventy years no fault has been detected. Thus the production of a faultless table is quite within the range of possibility; it is a matter of time, of care, of expense; and with our modern appliances the endless reproduction of the plates is easy; so that computers ought to be in possession of tables trustworthy throughout, especially of such tables as are of universal application.

Though not needed for the every-day work of the computer, tables of excessive precision are not the less needed in special departments, and in the preparation of other tables for ordinary use. Their extent and the expense of preparing them, coupled with the smallness of the number of those by whom they are desired, precludes their preparation by private parties, and relegates the matter to the care of public authorities.

In the same way that the "Nautical Almanac," which is far beyond the reach of private enterprise, and yet is needed for the advancement of navigation and astronomy, is undertaken by the Government, it would be right to carry out the idea of Davies Gilbert, and to confer, by the publication of exact tables, a similar boon upon the other branches of science.

It would be fitting that this should be done by the British Government, seeing that the invention and completion of the logarithmic method belong to the Island; and it would be not less fitting that the first public body to move in the matter should be the Royal Society of Edinburgh, from whose place of meeting

we could almost have seen the roof under which John Nepair elaborated his invention, and could fancy to have heard the creaking of the screw with which Andrew Hart imprinted the "Canon Mirificus."

Number.	Log. to 15 Places.	To 10.	Vlacq.	Vega.
20071	90109 36054	90109	90110	110
20280	79506 61298	79507	79506	506
20375	76174 12014	76174	76175	175
20645	48872 10721	48872	48873	873
20822	24421 41256	24421	24422	422
20866	92030 46050	92030	92031	031
21245	67354 25533	67354	67355	355
21749	92932 69231	92933	92932	932
21795	68733 53703	68734	68735	735
21904	34307 89915	34308	34309	309
22016	84165 55417	84166	84167	167
22200	29744 50639	29745	29744	744
22312	85012 57993	85013	85012	012
22877	90721 80887	90722	90721	721
22996	22999 73937	23000	22999	999
23274	10299 83700	10300	10299	299
23492	99921 70919	99922	99923	923
23820	17571 46759	17571	17572	572
24156	50209 36279	50209	50210	210
24580	18785 50435	18786	18785	785
25173	49758 10852	49758	49759	759
25524	87359 50354	87360	87359	359
25586	23955 50655	23956	23955	955
25707	13975 50452	13976	13975	975
26004	01573 67443	01574	01573	573
26407	90654 45820	90654	90655	655
26517	43886 18717	43886	43889*	886
26642	68239 65258	68240	68239	239
26699	49953 49034	49953	49954	954
26717	76904 57995	76905	76904	904
26728	64626 30075	64626	64646*	646
27291	94494 30434	94494	94495	495
27560	92132 35588	92132	92133	133
27586	87318 72159	87319	87318	318
27861	67002 67696	67003	67002	002
27921	09686 32521	09686	09687	687
28486	14699 52392	14700	14699	699
28680	91469 95763	91470	91469	469
29226	93799 55414	93800	93799	799
29446	63077 50861	63078	63077	077
29639	35467 49658	35467	35468	468
29703	03152 31285	03152	03153	153

2. Note on the Submerged Fossil Trees of Granton Quarry. By Sir R. Christison, Bart., Hon. V.P., R.S.E.

It may interest those who are acquainted with the history, structure, and composition of the Craigleith fossil trees, described in the two papers recently read to this Society, to learn that an opportunity has occurred for examining comparatively specimens from the submerged fossil trees of Granton Quarry. The specimens were preserved by Mr Hawkins, engineer of the Granton harbour, and have been, through his kindness, not only subjected to examination, but also presented for preservation to the Botanic Garden collection.

It turns out that the microscopic structure and chemical composition of the greater of the two Granton fossils are precisely the same with the structure and composition of the fossils of Craigleith, two miles distant. The embedding rock is also the same in composition, whether in its pure state, or where altered by percolating water. That is, the microscopic structure of the Granton fossils appears to be that of the pine tribe; and the fossilising material consists of the carbonates of lime, magnesia, and iron, all in notable proportion; and while the fundamental rock of the quarry is a very pure quartzy sandstone, without any binding calcareous carbonate, many masses may be seen among the blocks raised many years ago from the quarry, but not made use of, which like similar altered specimens from Craigleith, have their fracture, colour, and toughness changed by the same material which has fossilised the trees. In the fossils, too, there is the same three or four per cent. of charcoal left after the solvent action of acids on the fossils of Granton as on those of Craigleith.

That part of Mr Witham's fossil of 1830 which lay in front of the Museum of Science and Art, has now been removed to the Botanic Garden, to be added to the lower part of the trunk of which it is the continuation. In separating two of the segments, a cavity was found which contained a matter like charcoal, some fragments of which even presented the fibrous appearance of charcoal to the naked eye; and Mr Sadler, of the Botanic Garden, has ascertained that some of these fragments show before the

microscope the characteristic punctated structure of the vertical section of the pine family. This, I believe, is the first time that this particular part of the pinaceous structure has been observed in any of these fossils.

It may be farther noticed that there are now in the Botanic Garden Museum two great polished slabs, nearly three feet in diameter, from the Craigleith fossil last discovered,—one of which shows in many places to the naked eye the annual layers of wood concentrically; and that in breaking up a large mass of the same fossil, in the hope of discovering a deposit of charcoal in a cavity, several fine fractures were obtained, showing distinctly to the naked eye large surfaces of the ribbon-like structure of the transverse medullary rays, and one surface presenting to the naked eye not only these markings, but likewise the annual layers cut vertically.

3. Note on Grouse Disease. By Professor Maclagan.

The result of the author's examination of diseased birds has been to confirm the statements of Drs Cobbold and Crisp, lately published in the "British Medical Journal," that diseased grouse, or at least the emaciated birds commonly known as "piners," owe their depraved condition to a small thread-like worm (*Strongylus pergracilis*, Cobbold) which infests the cæca. The author concurs in the opinion entertained by most of those who have written on the subject, that the tape-worm (*Tænia calva*), which is well known to infest the grouse, is not the cause of the disease. The *Tænia* is undoubtedly often present along with the *Strongylus* in diseased birds, but is often found by itself in plump healthy grouse. The worst cases seem to be those in which both are present in quantity, as in one examined by the author, whose cæca was crowded with *Strongyli*, whilst the intestine contained ten tape-worms, the whole weight of the bird, a full-grown cock, being only 15½ ounces. It is not possible accurately to determine the number of *Strongyli* in any one case, but, so far as it could be determined, it appeared that the more numerous were the *Strongyli* in any one bird, the greater was its emaciation. By a rough but moderate calculation

the author was led to estimate the number of these worms in one of his birds as at least 4800.

There is, however, no definite line between birds with and those without this disease, for almost every grouse is the "host" of fewer or more of the parasites. Of eleven birds carefully examined by the author, with the aid of Mr Stirling of the University Anatomical Museum, in one only, a fine Irish cock weighing one pound eleven ounces, were none detected. The other birds examined were from various localities: those containing the fewest worms, and of the heaviest weight, were from Ireland and Orkney; those from Lanarkshire, East Perthshire, and Sutherland were the most affected by the worm and most emaciated.

So far the author concurs with the writers named above, that one form of grouse disease is this helminthiasis, due to the *Strongylus*, which destroys the birds by ultimately annulling the functions of the cæca, in which the real digestion of the birds' food goes on. The mucous membrane is not inflamed, but irritated, throwing off great quantities of large columnar epithelium, and instead of true fæculent matter, or remains of food, the intestines and cæca usually contain only a pinkish grey mucus. The cæca, however, seem occasionally to be softer and more easily torn than is natural. In none of these birds was any other morbid appearance found capable of accounting for their morbid state.

It is not yet clear to the author, however, that this helminthiasis is *the* disease which has so often swept the moors of Scotland and England. It is quite possible it may be so, and there is nothing in its rapid spread on particular moors in certain seasons to prevent its being due to parasites; but the author thinks that further inquiry is desirable, and, speaking as a sportsman, would suggest to the proprietors and tenants of moors, which are now so valuable as to be a subject of national importance, to raise by subscription a sufficient fund to enable them to commission some competent naturalist to work out the subject. The genesis of the worms, both *Strongyli* and *Tæniæ*, in a scientific point of view, irrespective of the hope of some practical conclusion, appears to be worth the expenditure of some money.

4. Latent Heat of Mercury Vapour. By James Dewar, Esq.

5. Notes by James Dewar, Esq. (1.) Problems of Dissociation; (2.) Formation of Allotropic Sulphur; (3.) Heat of Fermentation.

6. Further Note on Continuants. By Thomas Muir, M.A., F.R.S.E., Assistant to the Professor of Mathematics in Glasgow University.

In my paper on Continuants, recently communicated to the Royal Society, it was shown that the order of a continuant may be depressed if the first element of the main diagonal be unity, viz., thus:—

$$K\begin{pmatrix} & b_1 & & b_2 & & b_3 & \\ 1 & & a_1 & & a_2 & & a_3 \ldots \end{pmatrix} = K\begin{pmatrix} & b_2 & & b_3 & \\ a_1+b_1 & & a_2 & & a_3 \ldots \end{pmatrix}$$

and from the definition it is evident that

$$K\begin{pmatrix} & mb_1 & & b_2 & \\ ma_1 & & a_2 & & a_3 \ldots \end{pmatrix} = m\,K\begin{pmatrix} & b_1 & & b_2 & \\ a_1 & & a_2 & & a_3 \ldots \end{pmatrix}$$

Hence we have

$$K\begin{pmatrix} & b_1 & & (b_1+a_1)b_2 & & (b_2+a_2)b_3 & \\ 1 & & a_1 & & a_2 & & a_3 \ldots\ldots \end{pmatrix}$$

$$= K\begin{pmatrix} & (b_1+a_1)b_2 & & (b_2+a_2)b_3 & \\ b_1+a_1 & & a_2 & & a_3 \ldots\ldots \end{pmatrix}$$

$$= (b_1+a_1)K\begin{pmatrix} & b_2 & & (b_2+a_2)b_3 & \\ 1 & & a_2 & & a_3 \ldots\ldots \end{pmatrix}$$

$$= (b_1+a_1)\,(b_2+a_2)\,(b_3+a_3)\ldots\ldots$$

From this it is clear that, in virtue of the relation which has given rise to the name "continuant," continued fractions of a certain class may be transformed into simple fractions, with continued products for numerator and denominator.

A general theorem on this transformation is given by Stern in "Crelle's Journal," vol. x. (1833), p. 267; and by a quite similar method several allied identities have been reproduced in a paper recently read before the Mathematical Society of London. All of them, however, may be established much more easily by means of the above results. Thus—

$$1+\cfrac{d_1-e_1}{e_1-\cfrac{(d_2-e_2)\,d_1e_1}{d_1d_2-e_1e_2-\cfrac{(d_1-e_1)\,(d_3-e_3)\,d_2e_2}{d_2d_3-e_2e_3-\cfrac{(d_2-e_2)\,(d_4-e_4)\,d_3e_3}{d_3d_4-e_3e_4-\cdots\cdots}}}}$$

$$=\frac{\mathrm{K}\left(1\quad \frac{d_1-e_1}{e_1}\quad \frac{-(d_2-e_2)d_1e_1}{d_1d_2-e_1e_2}\quad \frac{-(d_1-e_1)(d_3-e_3)\,d_2e_2}{d_2d_3-e_2e_3\,.\,.\,.\,.\,.}\right)}{\mathrm{K}\left(e_1\quad \frac{-(d_2-e_2)d_1e_1}{d_1d_2-e_1e_2}\quad \frac{-(d_1-e_1)(d_3-e_3)d_2e_2}{d_2e_3-e_2e_3\,.\,.\,.\,.\,.}\right)}$$

$$=\frac{d_1\,.\,d_2\,(d_1-e_1)\,.\,d_3\,(d_2-e_2)\,.\,.\,.\,.\,.}{e_1\,.\,e_2\,(d_1-e_1)\,.\,e_3\,(d_2-e_2)\,.\,.\,.\,.\,.}$$

$$=\frac{d_1d_2d_3\,.\,.\,.\,.}{e_1e_2e_3\,\;.\,.\,.\,.}$$

which is the general result obtained by Stern, and which probably includes all the others.

I am indebted to Professor Cayley for the remark that any continuant may be expressed by means of a simple continuant. Thus dividing the second column by the first constituent of it which is not zero, and then multiplying the third row by the same, and so on through the remaining columns and rows in succession, we have

$$\begin{vmatrix} a_1 & b_1 & 0 & 0 & 0 \\ -1 & a_2 & b_2 & 0 & 0 \\ 0 & -1 & a_3 & b_3 & 0 \\ 0 & 0 & -1 & a_4 & b_4 \\ 0 & 0 & 0 & -1 & a_5 \end{vmatrix} = \begin{vmatrix} a_1 & 1 & 0 & 0 & 0 \\ -1 & a_2\frac{1}{b_1} & 1 & 0 & 0 \\ 0 & -1 & a_3\frac{b_1}{b_2} & 1 & 0 \\ 0 & 0 & -1 & a_4\frac{b_2}{b_1b_3} & 1 \\ 0 & 0 & 0 & -1 & a_5\frac{b_1b_3}{b_2b_4} \end{vmatrix} \times b_2b_4$$

And so generally

$$\mathrm{K}\begin{pmatrix} & b_1 & & b_2 & & & b_{n-1} & \\ a_1 & & a_2 & & a_3 & \ldots & & a_n \end{pmatrix}$$

$$= \mathrm{K}\left(a_1\,,\, a_2\frac{1}{b_1}\,,\, a_3\frac{b_1}{b_2}\,,\, a_4\frac{b_2}{b_1 b_3}\,,\, a_5\frac{b_1 b_3}{b_2 b}\,\ldots\,,\, a_n\frac{\ldots b_{n-4}b_{n-2}}{\ldots b_{n-3}b_{n-1}}\right) \times \ldots b_{n-3}b_{n-1}.$$

To this may be added the following as being derived in similar fashion :—

$$\mathrm{K}\left(a_1 x^{-1}, a_2 x\,, a_3 x^{-1}, a_4 x \,\ldots\, a_n x^{(-1)^n}\right) = \mathrm{K}\,(a_1\,, a_2\,, a_3 \ldots a_n)$$

if n be even, and

$$= \mathrm{K}\,(a_1\,, a_2\,, a_3 \ldots a_n)x^{-1}$$

if n be odd.

The simple continuant I have found to be identical with Euler's "*Novus Algorithmus.*" An examination of his paper with this title will at once make evident the advantages of the new mode of considering the function.

Monday, 4th May 1874.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read :—

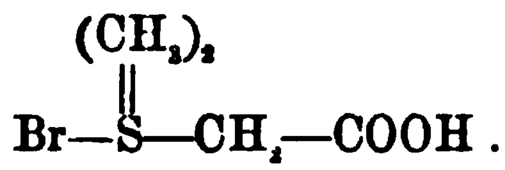
1. On the Formation of Allotropic Sulphur. By James Dewar, Esq.

2. On Some Compounds of Dimethyl-Thetine. By Professor Crum Brown and Dr E. A. Letts.

(*Abstract.*)

In this paper the authors describe in detail compounds of dimethyl-thetine, some of which were enumerated in an earlier communication. Hydrobromate of dimethyl-thetine is readily obtained as a colourless crystalline body by the action of sulphide of methyl on bromacetic acid at ordinary temperatures. Its analysis shows that it contains quantities of the different elements agree-

ing with the formula $C_4H_9BrSO_2$, and its mode of formation and reaction lead to the constitutional formula —



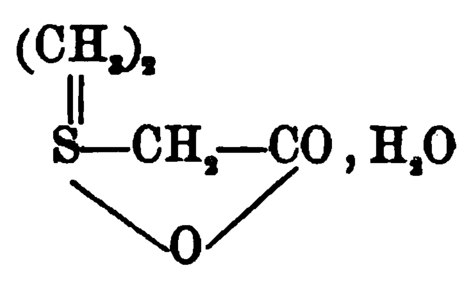
The result of its analysis are as follows :—

	Calculated.		Obtained.	
C_4 = 48	......	23·9	23·7	23·1
H_9 = 9	......	4·5	4·5	4·8
O_2 = 32	......	15·9	—	—
S 32	......	15·9	—	16·1
B_1 80	......	39·8	40·0	39·7
201		100·0		

It is a very deliquescent body, and has a powerful acid reaction.

Dimethyl-thetine.—The base, of which the body just described is the hydrobromate, is obtained from the latter by the action of oxide of silver. It may also be prepared from the sulphate of dimethyl-thetine by treatment with carbonate of baryta. It is a very deliquescent crystalline body, containing one molecule of water of crystallisation, which it loses if exposed for several days over sulphuric acid *in vacuo*.

The numbers calculated for the formula

$C_4H_8SO_2 , H_2O =$ 

agree with those obtained by experiment, thus—

Calculated in 100.		Obtained.	
C	34·7	34·0	33·9
H	7·2	7·3	7·3
H_2O	13·0	18·0	—

Hydrochlorate of dimethyl-thetine.—This body may be obtained either by saturating a solution of the base with hydrochloric acid, or by means of double decomposition between the sulphate of

dimethyl-thetine and chloride of barium. It is a crystalline substance of strong acid reaction.

The formula

$$C_4H_9ClSO_2 = Cl—\overset{(CH_3)_2}{\overset{\|}{S}}—CH_2—COOH$$

was verified by a chlorine determination.

Calculated in 100.	Obtained.
Cl ...... 22·7	23·4

Chloroplatinate of dimethyl-thetine is obtained in beautiful light orange-coloured crystals when solutions of the hydrochlorate and chloride of platinum are mixed. This salt contains two molecules of water of crystallisation, and has the formula

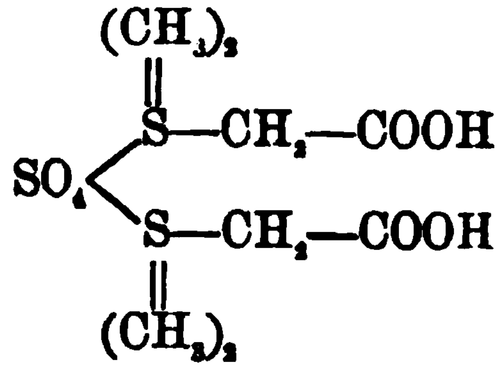
$$2(C_4H_9ClSO_2), PtCl_4, 2H_2O.$$

The salt was analysed by determination of water and platinum—

Calculated in 100.		Obtained in 100.	
Pt	= 28·6	28·5	28·4
H_2O	= 5·2	5·4	—

Bromaurate of dimethyl-thetine was obtained by mixing alcoholic solutions of hydrobromate of methyl-thetine and bromide of gold. The analysis shows too small a quantity of gold for the normal bromaurate, but agrees with the amount required for a salt crystallising with three molecules of alcohol. This compound has not been more particularly examined.

Sulphate of dimethyl-thetine was prepared by the action of sulphate of silver on hydrobromate of dimethyl-thetine. It can be obtained in large crystals, which are not deliquescent. It was analysed by a sulphuric acid determination.


requires 28·4 per cent. SO_4

whereas 28·2 per cent. and 28·0 per cent. were obtained by experiment.

Nitrate of dimethyl-thetine, prepared by treating the hydrobromate with nitrate of silver, is a transparent crystalline substance.

The formula

$$C_4H_9(NO_3)SO_2 = NO_3{-}\overset{(CH_3)_2}{\overset{\|}{S}}{-}CH_2{-}COOH$$

was verified by an organic analysis—

	Calculated.	Obtained.
C	...... 26·2	26·4
H	...... 4·9	4·9

Double Salt of dimethyl-thetine and bromide of lead.—A boiling solution of hydrobromate of dimethyl-thetine dissolves carbonate of lead with evolution of carbonic acid, and on cooling deposits beautiful silvery scales.

The formula of this body is

$$C_4H_8SO_2\,,\ 2PbBr_2$$

	Calculated.	Obtained.	
C	...... 5·6	5·7	5·7
H	...... ·9	·9	·9
Br	...... 37·0	37·0	37·5
Pb	...... 48·4	48·4	48·5

In addition to these compounds, a very beautiful salt was obtained by the action of hydriodic acid on the base, or by double decomposition between iodide of barium and sulphate of dimethyl-thetine. In appearance it resembles permanganate of potash, and is a poly-iodide; but its examination is not as yet completed.

Hydrobromate of diethyl-thetine was prepared in a similar manner to the corresponding dimethyl compound, which it resembles; it is so deliquescent, however, as to render its analysis almost impossible. It gives beautiful orange-coloured salts with bichloride of platinum.

In the course of these experiments the action of iodacetic ether upon sulphide of methyl was studied. The reaction here takes a different course, iodide of trimethyl-sulphine being produced in large quantity.

3. On a New Example of the Opheliidæ (*Linotrypane apogon*)* from Shetland. By W. C. M'Intosh.

This peculiar iridescent pinkish Annelid was dredged in 1871 in Bressay Sound, in four or five fathoms, on a bottom of coarse sand and gravel, which abounded with finely-branched *Melobesia calcarea*, Ell. and Soland.

The form resembled an active nematoid worm, being elongated, nearly cylindrical throughout the greater part of its length, and devoid of bristles or lateral projections. It progressed in the most vigorous and spasmodic manner, by twisting or thrusting itself through the sand, after the mode of *Ammotrypane*, or a most rapid eel-like fish. Moreover, the slightest interference caused it to break in pieces, so that not a single specimen out of the whole series remains entire, though every precaution was taken to immerse the animals in spirit on removal from the dredge. The activity and purpose displayed by the species are diagnostic when compared even with the most nimble of the nematoid group, so that no difficulty is experienced in distinguishing it.

The Annelid reaches the length of three or four inches, and is only about a millimetre ($\frac{1}{25}$th inch) in diameter. The body is rounded, slightly tapered in front, where the pinkish colour is best marked, and richly iridescent, even to a greater degree than either *Ammotrypane* or *Ophelia*. The head terminates in a rounded anterior border, from which two short clavate processes project. The latter have a very thin investment of the hyaline cuticle, with a thick layer of granular cells (hypoderm) beneath. Some longitudinal fibres occur at the base, but the contractility of the organs is limited. Two eyes, consisting of encapsulated masses of black pigment, are situated near the dorsal surface of the tissues of the snout.

* λινον, a thread, and τρυπάνη; the specific name from ἀπωγων, beardless

Body-wall.—The external investment is a translucent, perfectly smooth, glistening cuticle, very thin on the snout, cephalic processes and the anterior region, but of considerable thickness and great tenacity throughout the rest of the body. It is this layer which enables such forms to bear much strain in a longitudinal direction, and, by its great elasticity, to dispense with a special circular layer of muscular fibres. In some of the Nemerteans, for instance, where the cutaneous tissues are soft and easily injured, a very perfect circular muscular coat occurs next the basement-membrane of the latter, and exterior to the longitudinal layer. When a single layer of this hyaline cuticle is examined, after mounting in chloride of calcium, a number of puncta, arranged with greater or less regularity, and apparently passing quite through it, are found. By tearing with needles, or examination in simple water, it is further seen to be composed of a closely interwoven series of very fine fibres, many of which have a crossed-spiral, or oblique direction. This is a common arrangement in such iridescent forms. The cuticle readily separates from the subjacent layers in the preparations, a feature less evident in *Ammotrypane* and *Ophelia*. Beneath the foregoing is a cellulo-granular layer (hypoderm), which in transverse sections preserves a nearly uniform thickness, except inferiorly, where the nerve-cords occur.. The cells vary in size, are filled with granules, and embedded in a hyaline intercellular substance. Many granules also exist amongst the cells. In the cephalic region a considerable thickening of the coat takes place, especially inferiorly, and this enlargement coincides with the diminution of the hyaline cuticular layer formerly mentioned. A boundary or basement-layer occurs on the inner surface.

Within is a great longitudinal muscular coat, which (besides the passage of the oblique muscular fibres) is interrupted at two points in its circumference, viz., at the median line of the dorsum, and the opposite point inferiorly. The former is but a faint separation, caused by the suspensory fibres of the alimentary region; the latter is a boldly-marked hiatus—the inferior fibres of the alimentary canal, the oblique muscular bands of the body-wall, and the ventral blood-vessel meeting at this point. In ordinary transverse sections this coat presents a somewhat wavy, radiated appear-

ance, from the arrangement of the fasciculi. In stating that the direction of the muscular fibres in such sections is radiated, some explanation is perhaps necessary, for, while the fasciculi of the dorsal and lateral regions point more or less in this way, the arrangement at the raphe is different, since the oblique bands, passing down at an acute angle, direct, in the contracted state, the fasciculi upwards and outwards. They gradually become vertical, and then slant in the opposite direction, before leaving what may be termed the ventral region. A firm band, apparently of the limiting membrane of the hypoderm, proceeds from angle to angle at the raphe.

From this coat, at somewhat regular intervals, pass a series of muscular bridles, each forming a kind of diaphragm (dissepiment). Most of the fibres have a vertical direction. The same arrangement is observed in the Nemerteans and in most of the Annelida. Such bundles, of course, are altogether independent of the characteristic oblique bands of muscular fibres which pass from the lateral dorsal region on each side to the raphe at the ventral edge. Anteriorly the latter bands form, in contraction, a curve on each side, with the convexity directed inwards, and they enclose a somewhat elliptical portion of the great longitudinal layer, with a few cells and granules. The oblique bands spring from the basement-membrane, and thus pass through the longitudinal layer,—an arrangement very well seen in front, where the bands are of great thickness. Posteriorly the comparative slenderness of the oblique muscles makes this subdivision of the longitudinal layer indistinct, but it is nevertheless present. In this region, also, the distance between the middle of the oblique band and the longitudinal coat is considerable, the space being filled with cellular tissue and a few fibres.

It will thus be observed that the animal has a very complete muscular system, relatively of great power, for the execution of its remarkable boring propensities in sand and gravel.

Digestive System.—The mouth opens in the preparations on the ventral surface, a short distance behind the tip of the snout, and has prominent lips. It leads into a richly ciliated digestive chamber, which runs to the posterior end of the body. No dental organs of any kind exist, the food apparently consisting of sand

or sandy mud, requiring nothing more than simple engulfment. Anteriorly, what may be termed the œsophageal division of the canal has internally a well-defined margin, covered with closely-set cilia, the wall consisting of the usual granular gland-cells, embedded in a hyaline stroma, with muscular fibres. Posteriorly, it is more opaque and granular, and appears to end in an anus without processes. All the specimens, however, were imperfect. The organ is thrown into innumerable rugæ internally; while externally it is kept in position by the dorsal and ventral fibres formerly noted, as well as by the dissepiments. The broad inferior fibres pass to the transverse band at the raphe, and a few even extend in some sections to the exterior border of the cellular coat in this region, at the nerve-cords.

Nervous System.—It is somewhat difficult to make out the arrangement of the cephalic ganglia in the specimens; but they are situated in the snout, near the eyes, and form two slightly tinted masses, terminating on each side in a buccal cord, which passes downwards to the ventral surface, and extends along the body beneath the transverse band of the raphe. The cords are larger in front, and somewhat farther apart, but throughout the rest of the body are closely approximated. The usual granular sheath surrounds them, and they are also protected by part of the cellular coat inferiorly.

In comparing the foregoing form with the representatives of the *Opheliidæ* at present described, it is at once distinguished by the absence of bristles. In *Ammotrypane* the united nerve-cords are situated at the ventral edge of the T-shaped prolongation of the body-wall inferiorly, and have a muscular column between them and the perivisceral cavity. In *Ophelia* the nerve-cord lies within the great longitudinal muscular cord, at the junction of the ventral prolongations (in transverse sections). The body-wall differs in the relative thickness of the several layers, and especially in the great bulk of the cellular coat in the new form. One of its nearest allies seems to be a new *Ammotrypane* dredged in Valentia harbour by Dr Gwyn Jeffreys, which shows a very minute trace of bristles, though the form of the body closely agrees with the *Ammotrypane aulogaster* of H. Rathke. In the Irish species, however, the united nerve-cords lie between the ventral ends of the powerfully-

developed oblique muscular bands which separate the longitudinal coat in the median line inferiorly.

The occurrence of an Annelid proper devoid of bristles is an interesting fact; for, though such organs are feebly developed in *Tomopteris*, they have been considered on the whole so universal, that, for example, the two great divisions Polychæta and Oligochæta rest thereon. The new form likewise shows no trace of segmentation externally, in this respect agreeing with the Nemerteans, yet in structure it is truly an Annelid proper. It is difficult to assign its exact position at present, and the association with the Opheliidæ may be regarded as provisional.

4. The following concluding Remarks were made by Mr D. Milne Home, who occupied the Chair in room of the President:—

I. I have been requested by our Secretary to announce formally from the Council, that this is the last meeting for the Winter Session.

You will have seen from the billet, that our President, Sir William Thomson, was to have been in the chair to-night, and to have closed the session with some remarks suitable to the occasion.

The Council are much disappointed, and no doubt you also are; but I am more distressed than any one at Sir William Thomson's absence. There is a letter from him to the Secretary, dated on Friday last, mentioning that he could not attend this evening, as he expected to be in his yacht to-day in the Bay of Biscay.

The Council, therefore, had no alternative but to appoint me, as the only Vice-President at hand, to occupy the chair to-night. The occupation of the chair is unaccompanied by any difficulty,—but the other duty, of offering concluding remarks worthy of your acceptance, I find it simply impossible to perform. I am sure you will neither expect it, nor wish me to attempt it.

Such remarks, therefore, as I shall offer, will be matter of mere form, and will not contain thoughts or suggestions, or information of any scientific value.

II. I must, however, detain you for a few moments in adverting to our proceedings during the past winter.

1. I think from my recollection of the papers which have come before our meetings, that we have attended to most of the objects for which our Society was established.

We have had papers on the various physical sciences—Chemistry, Natural Philosophy, Geology, Botany, Mathematics, Anatomy, and Zoology. But besides science, our Society was intended for the encouragement of Literature; and I regret to say, that I do not remember any paper read this winter of a literary character.

At our last meeting, one of the papers was by Mr Sang on Logarithms, and he produced on our table no less than 20 MS. volumes of logarithms to 15 places of decimals! the publication of which, he pointed out, would be of great advantage to astronomers and others who require the aid of logarithms in their calculations, and accompnaied by minute accuracy. That opinion was publicly confirmed by other members of the Society very competent to judge; and I may now announce, that our Council have, in accordance with that opinion, come to the resolution of ascertaining whether Government will undertake the publication of Mr Sang's valuable tables, —for the cost would go far beyond our own resources as a Society.

2. I cannot conclude what I have to say of our Society, without adverting to the losses we have sustained by the death of several distinguished Honorary Associates, viz., Louis Agassiz, Lambert Adolphe Jacques Quetelet, Auguste de la Rive, and John Stuart Mill. Obituary notices of some of these distinguished men have already appeared in our Proceedings. The others will be noticed by our President in his Address, when the next session commences.

The Council have filled up these vacancies by selecting other eminent men as Honorary Associates; and these nominations have been confirmed by the Society.

Whilst referring to the list of our Honorary Fellows, I cannot avoid mentioning a name, which I see standing on the same page, and standing by itself, viz., *Sir Richard Griffiths.*

Sir Richard is, I believe, the oldest member of our Society. He will, in September next, have completed his ninetieth year. He was in my house two months ago, in good health, on his way to Ireland, where he is at present residing. It so happens that this

forenoon I had the pleasure of receiving a letter from him, accompanied by a short biographical memoir of his many scientific researches as a geologist, and of his great public services as a high officer of the Irish Government.

This memoir I have brought with me this evening, that you may see in it an excellent photograph of my venerable friend, and our oldest colleague.

III. Having said all that occurs to me of ourselves and our own doings as a Society, perhaps you will permit me, before closing, to allude to what is doing generally in the country for the advancement of science.

There are two aspects in which science may be viewed:—*First*, The teaching of what is known; and, *second*, The investigation of what is not known.

1. As regards the teaching of what is known,—

(1.) I must advert to the great additional impulse lately given in Scotland, England, and Ireland, by the Kensington Department of Science and Art.

The efforts of that department are confined chiefly to schools—Middle-Class Schools—though they do not object to assist science classes, even in elementary schools.

In Scotland, ten years ago, there were only 4 schools in connection with the department, now there are 118. I observe a list of 42 of these Scotch schools to which payments were made last year, amounting to L.1746, as perquisites to the teachers, which is no small pecuniary encouragement.

(2.) Another measure—not yet adopted, but which, if adopted, will probably conduce to the advancement of science teaching—is one about to be proposed to Parliament by a distinguished Fellow of the Society, Mr Lyon Playfair. He intends to ask the House of Commons to pass a resolution, recommending the Government to create a department for Education, Science, and Art, with a responsible minister at the head of that department.

I feel very sure that Mr Playfair will be able to make out a good case for such an appointment.

There is no country in the world whose various industries are more benefited than ours, by the help of science, and by that offi-

cial encouragement of science which is at present almost entirely awanting.

2. The other aspect of science to which I referred, is aiding in the investigation of truths not yet known.

In Germany there exist colleges of research, in which persons can get the use of instruments, and a laboratory for conducting experiments in any department of science to which they devote themselves.

Every one in the least acquainted with science must appreciate the importance of such institutions. In this country there are none, at least none except on a very small scale, and belonging to individual professors. But I am happy to say that there seems every prospect of this great desideratum being likely to be soon supplied.

The need of these institutions has been for some years pointed out by various scientific bodies, ourselves among the number; and the fruits of these expositions are now appearing.

(1.) Thus, I read in the "Scotsman" newspaper of Saturday last, that in *Glasgow* University such an institution is about to be formed. Principal Caird announces it in these terms,—terms which, whilst gratifying to Glasgow University, you will see that we as a Society also have reason to be proud of.

"A valuable gift has been made to the University by one of its most distinguished professors, Sir William Thomson, in conjunction with the representatives of Dr Neil Arnott. To a donation of L.1000 by Dr Arnott's widow, Sir William has added a sum of L.2000 for the endowment of the new office of Demonstrator in Experimental Physics, in connection with the chair of Natural Philosophy. By that endowment Sir William has conferred new obligations on the University, on which his great name reflects so much honour."—*Scotsman Newspaper*, 2*d May* 1874.

(2.) Another example I find from the *April* number of "Nature," in the following terms:—

"The magnificent sum of L.10,000 has been made over by the late Mr E. R. Langworthy to the Owens College, Manchester, for the purpose of developing the chair of Experimental Physics. The terms in which the bequest is made are so forcible and clear, that they deserve to be quoted:—'I bequeath to the trustees of Owens College ten thousand pounds. It is my desire that students may

be instructed in the method of Experiment and Research, and that science may be advanced by original investigation. I also desire that the professor appointed may be selected on account of his knowledge having been especially obtained by original investigation, and that his appointment shall be contingent upon the continuance of such investigation.'"

(3.) The third example I give is from the *March* number of the same newspaper.

"The Cavendish Laboratory.—This laboratory, in which every facility is furnished for the prosecution of physical research, is the munificent gift of William Cavendish, Duke of Devonshire, K.G., Chancellor of the University of Cambridge, who has intimated his intention of presenting it complete to the University.

"The building, which is now finished, was erected at an expense of about L.10,000.

"The laboratory is open daily from 10 A.M. till 6 P.M., under the superintendence of the professor of Experimental Physics, for the use of any member of the University who may desire to acquire a knowledge of experimental methods, or to take part in physical researches."

3. These measures are all intended to give greater facilities for original research. They indicate the strong belief existing in all thoughtful minds, of the importance of giving such facilities.

It is to be wished that this opinion may have impressed itself on the minds of the Royal Commissioners, who are about to issue their report on the aid which should be given in this country by the State to science.

It is a hopeful circumstance towards that view, that His Grace the Duke of Devonshire, to whom I have just alluded, is at the head of that Royal Commission. His munificent gift to Cambridge University shows how well His Grace knows what is necessary for the advancement of science in this country.

IV. With these remarks, gentlemen, I now declare the Winter Session of our Royal Society closed; and I only farther express a hope that all of us here may be spared, and many more of our colleagues also, to meet again at the commencement of our next Winter Session.

The following Gentlemen were elected Fellows of the Society:—

John Chiene, M.D., F.R.C.S.E.
Joseph Bell, M.D., F.R.C.S.E.
E. A. Letts, Ph.D., Assistant to the Professor of Chemistry in the University of Edinburgh.
Baden Powell, Esq., Conservator of Forests in the Punjab.

Donations to the Royal Society Library during Session 1873-74:—

I. Authors.

Archer (William Henry). Abstracts of English and Colonial Patent Specifications relating to the Preservation of Food. Melbourne, 1870. 8vo.—*From the Author.*

Arrest (Dr H. d'). Indbydelsesskrift til Kjbenhavn Universitets Aarsfest til brindring om Kirkens Reformation. 4to. 1872. —*From the Author.*

Beetz (W.). Der Arnheil der K. bayerischen Akademie der Wissenschaften an der Entwickelung der Electricitätslehre. Munich, 1873. 4to.—*From the Author.*

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Memoirs of the Survey of India. Palæontologia. Vol. IV. Nos. 3, 4; Ser. VIII. Vol. I. No. 1; Ser. IX. 4to.—*From the Survey.*

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OF THE

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VOL. VIII. | 1874–75. | No. 90.

Ninety-Second Session.

Monday, 23d November 1874.

Sir WILLIAM THOMSON, President, in the Chair.

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Monday, 7th December 1874.

SIR WILLIAM THOMSON, President, in the Chair.

The Keith Prize for the Biennial Period (1871–73) having been awarded by the Council to Professor Tait for his Paper, entitled "First Approximation to a Thermo-Electric Diagram," which has been published in the Transactions, the Medal was delivered to him by the President at the commencement of the meeting.

The PRESIDENT said—I have now the pleasing duty of awarding the Keith Medal, for 1871–73, to Professor Tait, for a paper entitled "First Approximation to a Thermo-Electric Diagram." This paper is published in the number of the Transactions which I hold in my hand, being Part I. of Volume XXVII., for Session 1872–73. The Society, considering the remarkable interest which attaches to the subject of this paper, and the very important discovery which it contains, will, I am sure, pardon me if I take up a little of their time by referring to some of the antecedents of Professor Tait's investigation. In the first place, there was the great discovery by Seebeck, about the year 1821, of thermo-electric currents. According to this discovery, if a circuit be formed of two different metals, and if the two junctions be kept at different temperatures, an electric current will be found to go round the circuit:—that is Seebeck's great discovery. Quickly following upon it was the very remarkable discovery made by Cumming, the late professor of chemistry in the University of Cambridge, that there is in certain pairs of metals an inversion of the thermo-electric current of a very remarkable kind. A few words are necessary to make it intelligible. Take a circuit of, let us say, bismuth and antimony. Let one of the junctions be kept at an ordinary temperature. Let the other junction be gradually raised in temperature. With a proper instrument for measuring the strength of the current flowing through the circuit, it will be found that the strength of the current gradually increases as the hot junction is made hotter and hotter. That was part of Seebeck's discovery.

Cumming experimented on many other pairs of metals, and, amongst them, copper and iron. In experimenting upon copper and iron, he found that as the hot junction is made hotter and hotter, the current increases, but only up to a certain limit. When the hot junction is made still hotter than a certain critical temperature, the current begins to diminish in strength, till it becomes zero. When the hot junction is made hotter still, the current becomes reversed. This was a very great discovery indeed in thermo-electricity. I could be tempted to go into some more details, but I should perhaps tax the patience of the Society were I to do so. But I cannot refrain from mentioning the thermo-dynamic relations of this discovery and the collateral discovery by Peltier, that there is a thermal effect produced at a junction of two different metals when by any means an electric current is caused to cross from one metal to another. I call that a collateral discovery,—collateral with Seebeck's, because it might have been made quite independently of Seebeck's. That might be one discovery and Seebeck's another, thoroughly disconnected, had we not light from theory to put them into relation with one another. The first ray of light thrown on the subject is to be found in an almost casual remark made by Joule in the course of some not casual—but thoroughly worked-out—observations upon the relations between the generation of heat on the one hand and the development of work from heat on the other. He remarked that the absorption of heat or the production of cold, manifested under certain circumstances by electric currents crossing a junction of dissimilar metals in Peltier's discovery, was to be looked to as a source of the power to be developed from a current of electricity produced by a thermo-electric action. The thorough working out of this remark of Joule's required the application of Carnot's theory. The relation between heat absorbed in one junction of the thermo-electric circuit, heat evolved in the other, and work done by the current in any instrument driven by the current put into the circuit—let us say, a mechanical engine raising weights, driven by the current—the relation, I say between heat absorbed in the one junction, heat developed in the other, and mechanical work performed, was fully worked out by the application of Carnot's theory. The introduction of Cumming's inver-

sion led to a very novel idea regarding the properties of matter, and, I may say, a novel discovery of action in connection with heat and electricity. To this action the name of the electric convection of heat has been given. When this is taken into account in connection with the discoveries of Peltier, Cumming, and Seebeck, it appears that the heat supply by which an engine driven by the thermo-electric current gets its energy may be not in a junction between two dissimilar metals, but by the peculiar absorption of heat, or generation of heat, which the discovery I have last mentioned points at when a current passes from hot to cold or cold to hot in the same metal. I am afraid the subject is somewhat involved; but I can only say that if the Fellows of the Royal Society diligently read their own Transactions, it would not be necessary for me to speak about it, because it was very minutely unfolded in a paper published a good many years ago. I chanced to be the author of that paper myself, and I speak just now, I must confess, with a considerable pride of the position it bears in relation to Professor Tait's discovery. This discovery is, that there are in many cases no doubt, but notably in cases in which iron is one of the metals, not merely one neutral point, as discovered by Cumming, but several neutral points. Tait finds that when a circuit, composed of iron and (let us suppose) platinum, has the temperature of one junction gradually elevated, while that of the other is maintained constant, the current first increases, then diminishes, then increases, then diminishes,—or, it may be, first increases, then diminishes to nothing, and goes in the contrary direction, attains a maximum in that contrary direction, then diminishes from that maximum to zero, and increases in the first direction to a maximum, and so on with every alternation. "Multiple neutral points" is the shortest name I can give in words to the great discovery in Professor Tait's paper. But I would do Professor Tait great injustice, and give you a very imperfect idea of the substantial character of the investigation for which the award has been made, were I to lead you to suppose that it was merely a discovery which some of you might for a moment imagine could be hit off by chance, and by an experimenter who had not his eyes absolutely closed. This discovery was made in the course of an elaborate investigation of which

results have been published from year to year, and generally within the two years for which the award has been given, in the Proceedings of this Royal Society. From year to year Professor Tait had accurate measurements of thermo-electric currents executed in his laboratory. The results he tabulated and represented graphically; and even before making this discovery, he had made some exceedingly important contributions towards the laws of thermo-electric force in various combinations of metals. It was in pursuing this elaborate investigation that he came upon the very astounding discovery—I call it really astounding—that there are several consecutive neutral points between one and another of certain pairs of metals—iron, platinum, iridio-platinum. It is not my object to give you imperfectly a small part of the information contained in this paper of Tait's. I desire rather to call your attention to a thing of much value which you have in your Transactions. But I may be allowed the pleasure of referring to the relation which my own previous investigation bears to this discovery of Tait's. I did succeed in doing away with the Peltier effect at one junction; I showed a thermo-electric current without the Peltier effect at one junction but with Peltier effect at the other junction, and the counter effect, absorption or evolution as it might be, made by my own thermo-electric convection in the homogeneous parts of the circuit. Now, Tait gives us a circuit in which there is no Peltier effect at all, and in which the whole origin of the power is to be found in absorption on the one hand and evolution on the other through electric convection of heat. I am afraid I have taxed your patience too long, because these matters have been reported in the Proceedings of the Society, and we have now a combined statement of the results in this paper. Before concluding, I wish to call your attention to the title of the paper, "A Thermo-electric Diagram," and the admirable manner in which Professor Tait has represented those results graphically to the eye, and the good use he has made of that graphical representation in aiding him to work out rigorously the laws of the phenomenon. I am sure the Society will feel very great satisfaction in the award which has been made on the present occasion. (To Professor Tait) I have now much pleasure in presenting you with the Keith Medal. If it were necessary

that there should be an inducement to you to continue your labours,—I cannot say with increased zeal,—but with zeal equal to that with which you have carried them on for the last few years, let me hope that the presentation of this medal may form a small part at least in such a motive.

The President opened the Session with an Address on Stability of Steady Motion.

The following statement respecting the Members of the Society was read by the President:—

I. Honorary Fellows—

Royal Personage, ...	1	
British Subjects, ...	20	
Foreign, ...	30	
Total Honorary Fellows, . .		51

Foreign Honorary Fellows Deceased since Session 1872–73: — Agassiz, Elie de Beaumont, Guizot, Hansteen, Quetelet, and De la Rive, ... 6

II. Non-resident Member under the Old Laws, . . . 1

III. Ordinary Fellows—

Ordinary Fellows at November 1873, ... 346

New Fellows, 1873–74.—Dr John Anderson; W. F. Barrett, Esq.; Dr Joseph Bell; Thomas Muir, M.A.; Dr Benjamin Carrington; Dr John Chiene; William Durham, Esq.; William Ferguson, Esq.; Dr Alexander Hunter; A. Forbes Irvine, Esq.; Francis Jones, Esq.; Dr E. A. Letts; James Napier, Esq.; T. B. Sprague, M.A. Cantab.; Dr R. H. Traquair; Dr J. Batty Tuke, ... 16

—— 362

Carry forward, 362

	Brought forward,	362
Deduct Deceased—Professor Thomas Anderson; George Berry, Esq.; Adam Black, Esq.; Sheriff Cleghorn; The Right Hon. Lord Colonsay; Francis Deas, Esq.; Henry Dircks, Esq.; William Euing, Esq.; Dr Robert Grant; Professor John Hunter; Professor Cosmo Innes; Charles Lawson, sen., Esq.; and Henry Stephens, Esq., ...	13	
Resigned—Dr W. M. Buchanan, Dr Alex. Wood, .	2	
Cancelled—Alfred R. Catton, Rev. Dr Hodson, .	2	
		17
Total number of Ordinary Fellows at Nov. 1873, .		345
Add Honorary and Non-Resident Fellows, . . .		52
Total to 1874,		397

Monday, 21st December 1874.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. Remarks on the Great Logarithmic and Trigonometrical Tables computed in the Bureau du Cadastre under the direction of M. Prony. By Edward Sang.

The volume marked "Logarithms O," now placed on the Society's table contains, to 28 places, the Logarithms of all numbers up to Ten Thousand. In preparing it, care was taken that each prime should be placed in connection with two distinct sets of other prime numbers, in order that all likelihood of mistake should be avoided. This amounts to the computation of each logarithm by two independent processes. Opposite each prime, reference is made to the page of the record of calculations; and the accompanying volumes marked "Construction, I., II.," contain minutes of each articulate

step, so that the genesis of any logarithm may readily be traced and examined.

The logarithms of the composite numbers have been found by addition; the greater part of these have been twice computed, but the whole await a revision in the manner about to be described.

Though very unlikely, it is yet possible that the same error may happen in each of the two calculations; thus in computing the logarithm of 8447, two distinct formulæ were used, namely—

$$2 \cdot 3769 \cdot 10^8 + 1 = 3 \cdot 37 \cdot 251 \cdot 3203 \cdot 8447$$
$$643 \cdot 10^7 - 1 = 3 \cdot 89 \cdot 2851 \cdot 8447$$

and the agreement of the two results would show conclusively both to be right, were it not for the possibility of a coincidence of error in some of the other logarithms, or of an error in the logarithm of 3 which is common to both.

In order to remove this exceedingly slight doubt, I prepared an index on movable cards, to show the page on which each formulæ is found; and another index, laid on the table, to show the formulæ into which each prime enters; this list, however, being limited to the first five occurrences. The above prime is there found in two more combinations:—

$$2017 \cdot 10^6 + 1 = 73 \cdot 3271 \cdot 8447$$
$$8447 \cdot 10^4 + 1 = 3 \cdot 7 \cdot 11 \cdot 37 \cdot 9883,$$

which also consist with previous determinations. Such coincidence removes all doubt as to the accuracy of the logarithm of 8447, excepting, indeed, the last-place uncertainty incident to all computations of incommensurable quantities.

To put the last touch to the trustworthiness of this table, I had begun to supply, from the larger primes downwards, a third formula to each prime having only two marked against it; that is, to place each prime in at least three distinct combinations, with the intention, when this revision shall have been completed, to cause the whole of the logarithms of the primes to be re-extracted from the minutes, and the summations to be redone by another hand; when my attention was drawn to a note in the scientific periodical "Nature," in reference to the proposed publication of the Million

Table for which this Vol. O, though then incomplete, had served as the foundation. This note I recite in full:—

Note from "Nature," 8th October 1874.

"The President and Council of the Royal Society of Edinburgh, impressed with the conviction that the progress of the sciences demands, and has long demanded, fuller and more exact tables of logarithms than any which at present exist, have memorialised Sir Stafford Northcote with the view of inducing the Government to print a nine-figure table of logarithms from unity to a million, part of which has already been calculated by Mr Sang, who has carried a fifteen-figure table up to 300,000. The subject of undertaking the publication of logarithm tables—so long as the number of figures does not exceed ten, the limit of utility—is one well worthy the attention of the Government; but in the present case there are several reasons why, if the application is refused, the loss to science will not be so great as some might think. In the first place, a table of 1800 large pages, whether in one, two, or three volumes, will be so unwieldy that, notwithstanding the ease of the interpolations, it would probably be very seldom used by computers; and secondly, because all who require more than seven figures will, no doubt, prefer to use ten, and consult the existing works. In fact, nearly all computors would, we believe, employ Vlacq or Vega in preference to the proposed table. Mr Sang, in the pamphlet which accompanies the memorial, makes a remarkable error when he intimates that the great French tables have not been used to verify any seven-figure table, so that, 'up to the present moment we have no verification of Vlacq's great work.' In point of fact, the whole of Vlacq was read with the copy of the French tables, at the Paris Observatory, by M. Lefort, and the results are published in Vol. IV. of the 'Annales de l'Observatoire de Paris.' Almost all the errors found by Mr Sang, by means of this table are among those there given by Lefort, and any one who choses can, without much expenditure of trouble, render his copy of Vlacq all but free from error—much more accurate than any new table could possibly be."

In my paper on last-place errors in Vlacq, read here on the 20th April, I say, speaking of the *Tables du Cadastre*, "I have not learned that these computations have been used for the verification

of those already printed, ... " and I have now to thank, very cordially, the author of the note for having mentioned the labours of M. Lefort, and so put me in the way of obtaining very startling and very important information in regard to Prony's Tables.

Concerning the errors in Vlacq, the author of the note says, "Almost all the errors, found by Mr Sang by means of this table are among those there given by Lefort," and deduces from this fact, in accordance with the well-established axiom that the part is *greater* than the whole—the futility of this or of any other new table. Not having yet mastered the first principles of this system of logic, I shall not venture to discuss any of the opinions of its inventor, and shall only look at the wisdom of his conclusion.

The studious investigator living in Iceland, in Terra del Fuego, or even here in Edinburgh, where logarithms had their birth, needing an extensive table of logarithms, must apply to his bookseller for Vlacq; the book has been "out of print" for two hundred years; if found at all, its price is antiquaries' price. Having succeeded, his next business is to procure a copy of the volume of the "Annales de l'Observatoire." I myself have tried the libraries here in vain, so that "without much expenditure of trouble," I have not made my copy of Vlacq "*all but* free from error,—much more accurate than any new table could *possibly* be." Even after I shall have effected the correction by help of Lefort's list, there will remain a great uncertainty, arising from the fact that two copies of Vlacq may not be in accordance with each other. To understand this, we may turn to the Errata printed on page 64 of Taylor's Tables. There we find, among others, the following remarks:—

"In about 100 copies; in about 120 copies; doubtful whether a few copies are erroneous or not; in about half the impression; only in one copy; and so on."

The movable types had been drawn out by the inking dabber, and erroneously replaced by the pressman. But in this case there is another uncertainty. Complaints were made of pirated editions, *fac simile* of the original Vlacq.

The search for the list of errors mentioned in the above note, led me to find two papers by M. F. Lefort. The first of these, contained in the "Comptes Rendus," tome xliv. page 1097, was

read to the Academy on the 25th May 1857, and entitled "Note sur les erreurs que contient une des Tables de Logarithmes de Callet, par M. F. Lefort," shows its author to possess every claim to our confidence and respect as a computer. The indication of wholesale errors in Callet's twenty-place table (copied also into Hutton's) is invaluable, as preventing farther mistake, and as showing the absolute need for a revision of our most trusted tables.

The second paper is in vol. xlvi., and was occasioned by remarks on the presentation to the Library of the Institute of a manuscript copy of the great tables which had been in the possession of Prony himself. It was presented by Prony's heirs, at the meeting of the Academy, on Monday the 17th May 1858.

In the course of remarks after the presentation, M. Elie de Beaumont expressed his opinion that the best means of preserving the work would be to print it; and M. Leverrier, drawing attention to M. Lefort's labours, spoke as if a doubt exist even as to the whereabouts of the veritable original calculations.

At the next meeting, that of the 24th May 1858, the note above mentioned was read, entitled, "Note sur les deux exemplaires manuscrits des Grandes Tables logarithmiques et trigonométriques, calculées au Bureau du Cadastre, sous la direction de Prony; par M. F. Lefort."

I regret that the length of this most remarkable and most valuable memoir precludes its reproduction here. As, however, the "Comptes Rendus" are very widely distributed, a verification of my remarks upon it is within the reach of many. The note refers almost exclusively to the logarithmic part of the tables.

It tells us that the great work was accomplished by a staff of computors, divided into three sections; the first section consisting of four or five geometers, whose business was to do the purely analytical part, and to calculate certain fundamental numbers; the second section was formed of seven or eight calculators acquainted with analysis: they made calculations directly from the formulæ arranged by the first section. The third section contained seventy to eighty persons having a very slight acquaintance with mathematics, whose business was to perform the additions and subtractions prescribed by the second section.

We have here a little army, with its generals and lieutenants, M. Prony himself being commander-in-chief. Lefort does not

acquaint us with the duties undertaken by the director, neither does he indicate the nature of the logarithmic formulæ which needed the concurrence of four or five geometers for their establishment. The actual business described by him begins with the doings of the second section, composed of "sept on huit calculateurs, possédant l'analyse et ayant une grande pratique de la traduction des formules en nombres."

These calculated 1°, the ten thousand first logarithms to nineteen decimals.

In regard to this part of the work, M. Lefort supplies us, somewhat indirectly however, with some very distinct information. He says (page 996) that Prony had borrowed a copy of Briggs' *Arithmetica Logarithmica*, which he returned enriched with an "*errata*," preceded by a note to the following effect;—

"This 'errata' is composed 1° of that which is at the top of the Latin introduction (of Briggs); 2° of the faults found by the citizens Letellier and Guyétant, calculators in the Bureau du Cadastre, on collating Briggs' table with the great tables of the Cadastre. These latter faults are marked by (a particular sign) the sign *."

Lefort goes on to say that this collation brought out thirty-two new faults, of which, however, he finds that *four* belong to the Cadastre table, leaving twenty-eight only to Briggs. Farther on he tells us that all the corrections for numbers above 10,000 refer to figures within eleven decimal places, leaving the twelfth, thirteenth, and fourteenth places of Briggs unchecked. From this I understand, although it be not explicitly so stated, that within the ten thousand, even these figures in Briggs had been examined.

Now, here I shall take the liberty of making an interjection of my own.

In March, while comparing my fifteen-place table with Vlacq, I thought it desirable to examine also the last places of Briggs; so the final groups of four figures in the *Arithmetica Logarithmica* were read with the corresponding figures in the volume now on the table, up to 3000. The readers found the labour of recording the discrepancies so great that they had to content themselves with a pencil mark when the difference was only unit, and with

a double mark when the divergence was more. Moreover, since perusing M. Lefort's note, I have caused the same readers to examine the tenth thousand, and I exhibit the book itself with their pencil marks. On counting these, it is found that considerably over forty per cent. of Briggs' final figures are in error.

When we consider that Briggs used only fifteen decimals in his "Tabula inventioni Logarithmorum inserviens," we can hardly expect a smaller proportion of errors than this among his final figures; and the examination exhibits in a very strong light the scrupulous care bestowed by him upon the work. But M. Prony's coadjutors of the second section carried their operations to nineteen decimals—that is, to one hundred thousand times the exactitude aimed at by Briggs, and not one of his errors should have escaped detection. It becomes, then, quite a mystery how MM. Letellier et Guyétant should have allowed upwards of four thousand errors to escape their notice.

To resume. Having accomplished this first part of their labour, the members of the second section computed 2° "the logarithms from 10,000 to 200,000 by intervals of 200 to 14 decimals, and with four, five. and even six orders of differences. The number of decimals was successively augmented by two for each order; so that, for example, the sixth difference was written with 26 decimals;" and this seems to have concluded their labours so far as the logarithmic table is concerned. The results were handed over to the third section.

Again, leaving for a while the course of M. Lefort's details concerning the preparation of the ruled sheets, I shall begin in earnest the computation of the final table. The logarithm of 10,000, and the differences of the successive orders, are inscribed on the upper horizontal line of a sheet, as in the annexed example. Our business is to add the first difference to the accompanying logarithm; to take the second difference from the first; the third from the second, and so on; and this has to be repeated 200 times, until we reach the number 10,200, whose logarithm, with its differences, ought to agree with what has been prepared for the second sheet. There are then to be performed two hundred additions and one thousand subtractions of large numbers before the calculator of the third section arrive at a check on, or can know anything of,

the accuracy of his work. Also, an error in the determination of the first difference of the sixth order is augmented 82,472,326,300 times in the final logarithm. Perhaps it is on this account that the differences are carried two places further at each step.

I do not understand in what way the two additional figures belonging to one order of difference are disposed of when subtracted from the difference of the next lower order, and M. Lefort gives us no information on this most perplexing subject. Putting the embarrassment therewith connected out of view, the performance of these twelve hundred operations, subject at every step to the prodigious accumulation of early error, is a task which, I venture to affirm, was never successfully accomplished by any computer.

This, however, is but a small part of the difficulty. In order to make the matter clear, I have placed in the accompanying scheme the logarithims of the first ten numbers, with their differences arranged in the manner described, as they ought to be found on the first sheet, each of them true to the last figure.

N.	Log.	1st Diff.	2d Diff.	3d Diff.	4th.	5th.	6th.
10000	·00000 00000 0000	4 34272 76862 7	43 42076 382	868 19823	26036 83	10 4100	[illegible]
01	04 34272 7686	4 34229 34786 3	43 41208 184	867 93786	26026 42	10 4048	[illegible]
02	08 68502 1165	4 34185 93578 1	43 40340 246	867 67760	26016 02	10 3996	[illegible]
03	13 02688 0523	4 34142 53237 9	43 39472 568	867 41744	26005 62	10 3945	[illegible]
04	17 36830 5846	4 34099 13765 3	43 38605 151	867 15738	25995 23	10 3893	[illegible]
10005	21 70929 7223	4 34055 75160 1	43 37737 994	866 89743	25984 84	10 3841	
06	26 04985 4739	4 34012 37422 1	43 36871 096	866 63758	25974 45		
07	30 38997 8481	4 33969 00551 0	43 36004 459	866 37783			
08	34 72966 8536	4 33925 64546 6	43 35138 081				
09	39 06892 4991	4 33882 29408 5					
10010	43 40774 7932						

There we find that the sixth difference is rapidly becoming less; at the top of the page it is 5202, and at the end of the 200 operations it should shrink to 4619. Now the third class computer, to whom the ruled sheet with only the first line inscribed on it was delivered, must necessarily have carried the same sixth difference all the way down, and therefore, even on the most favourable supposition that all the differences had been carried out to the twenty-sixth place, the result must have been egregiously erroneous.

Again, we come inevitably to the number 10010. Now the

essential character of the denary system of logarithms, that from which it derives all its advantages, is this,—that the mantissa of the logarithm of such a number as 10010 is an exact copy of that for 1001; but this number 1001 has had its logarithm already computed to nineteen places, so that we have only to collate the two in order to verify this much of the work. That is to say, this great unwieldy gap of 200 intervals had already been divided into twenty smaller gaps, and the labour of the interpolation has been uselessly augmented at least one hundred times.

And yet further, in the accompanying second scheme I have put the logarithms of the first ten numbers on the first sheet of the Cadastre manuscript, true all to the fourteenth place, with their differences of the first, second, and third orders. Differences of the fourth order only make their appearance in the sixteenth decimal place, and are here awanting. Fourteen place logarithms, then, for numbers above 10,000 can have no differences of the fourth, fifth, and sixth orders; and all this display of high orders of differences and of additional places of decimals has been a matter of pure supererogation.

N.	Log.	1st Diff.	2d.	3d.
10000	·00000 00000 0000	4 34272 7686	43 4207	86
01	04 34272 7686	4 34229 3479	43 4121	86
02	08 68502 1165	4 34185 9358	43 4035	89
03	13 02688 0523	4 34142 5323	43 3946	85
04	17 36830 5846	4 34099 1377	43 3861	87
10005	21 70929 7223	4 34055 7516	43 3774	87
06	26 04985 4739	4 34012 3742	43 3687	87
07	30 38997 8481	4 33969 0055	43 3600	86
08	34 72966 8536	4 33925 6455	43 3514	87
09	39 06892 4991	4 33882 2941	43 3427	86
10010	43 40774 7932	4 33838 9514	43·3341	
11	47 74613 7446	4 33795 6173		
12	52 08409 3619			

Here the third differences are, as it were, constant; the irregularities shown by them are, as every calculator knows, due to the neglect of the farther decimal parts. The third differences, if absolutely true, should show a slight steady diminution, and thus we may expect, without any calculation, that the two succeeding third differences should be from 88 to 85. Now the logarithm

of 5006 had been computed to nineteen places, and if we write the logarithm of 2 on the edge of a piece of paper, and then appose it to that of 5006, we shall obtain at a glance the logarithm of 10012. However, we shall not even take the trouble of this addition, but shall content ourselves with the fourteenth figure, which we find to be 9. Let us write this 9 in its proper place, which is two lines below. All that remains for us is to discover what two digits must be written in the last place of the third difference in order to produce this digit 9, knowing also that these digits should be about 6.

The strict investigation is simplicity itself. Let us put a, b, c, d for the last numbers in the successive columns a_1, b_1, c_1, d_1, for those which are to succeed a_2, b_2, c_2, d_2, for the next again, and we find

$$a_2 - a - 2b + 3c = 3d_1 + d_2 .$$

In resolving this equation we need attend only to the last figures, so that, applying it to the example before us,

$$\cdots 9 - 2 - 2 + 2 = 3d_1 + d_2, \text{ or}$$
$$\cdots 7 \qquad\qquad = 3d_1 + d_2.$$

Recollecting that d_1 and d_2 must be absolutely or nearly alike, we may put for trial

$$\cdots\cdots 7 = 4\, d_1,$$

whence d_1 is nearly 7, and we have actually $d_1 = 7$, $d_2 = 6$; so that the succeeding pair of differences must be 87, 86. Writing these in their places, we readily compute the next pair of logarithms as shown by the slender figures.

By a computation performed mentally in a few seconds, without putting pen to paper, we thus compute the third differences for the next pair of logarithms, with the certainty that the alternate one is absolutely true in the last place, and with a very slight uncertainty as to the intermediate. The whole magnificent array of fourth, fifth, and sixth differences, with decimals to the twenty-sixth place, disappears.

This amounts to the recognition of the fact that the large gap of 200 intervals had already been supplied with 100 stepping-stones, a fact of which neither Prony nor any one of his regiment

of geometers seems to have been aware. The method followed in the calculation of the Cadastre table of logarithms was an egregious blunder. The result was in accordance with the method.

After having told us that, for numbers above 10,000, the errors in Brigg's last three figures are not given, M. Lefort goes on to say (page 997):—"For all that there do exist numerous divergencies in the thirteenth and fourteenth places. We must thence conclude that the calculators did not suppose the Cadastre tables to have enough of precision for correcting Brigg's last places. In that they were perfectly right. The bases of the calculation had been chosen so as to make sure of twelve places, and the precautions taken were only applicable to the research of these twelve places, which are all that are fit for publication."

In order, after the method, but not behind it in importance, comes the conscientiousness with which the work is performed.

The careful computer who may have to revise his own work puts the first performance aside, even leaves it for a considerable time, lest the sight of the figures, or the remembrance of them, should lead to the repetition of an error: better still, he arranges the operation in another way, so that the same additions and subtractions may not recur. But when he has to do with hired assistants he must contrive safeguards against carelessness, even against simulation. Against the former it is possible, though difficult, to protect one's self, but against the latter there is no protection other than in repeating the work, which comes, in effect, to the dismissal of the delinquent. The most obvious safeguard is to have the work done by two or more computers who have no opportunity of intercommunication.

Now the above described fundamental arrangement of the work excluded the possibility of such isolation. The computer—a very ordinary computer, knowing little of mathematics—was desired to make a chain of calculations involving about 13,000 figures; and the test, to him, of the accuracy of his performance was to be found at its conclusion. By the time that the twentieth part of this task was done, the calculator must have felt the danger of error; perhaps he revised thus far on loose paper, found errors, and corrected them. Another division of the task was done in

the same way, and, with trembling, the conscientious worker having toiled for several days, arrived at the end, to find, in spite of all his care, some lamentable errors. The search for the source of these, and their correction, might occupy more time and be more irksome than the actual work. But his collaborator had experienced the same troubles. In order to lessen these, and really to improve the performance, the two came to exchange their loose sheets; and the habit of working on separate papers from which copies were made upon the official sheets came to be firmly established. This was inevitable, and might easily have been foreseen.

On page 998, M. Lefort details circumstances which seem to him to prove that the majority, if not the totality, of the calculators of the third section made their additions and subtractions on loose leaves, which they could dispose of freely; afterwards writing the results on the ruled official paper. He remarks, "This was incontestably a fault." It was, in my opinion, a complete relinquishment of the safe-guard afforded by duplicate manipulation. Now this gross and habitual infraction of the official rules could not have remained hid from the superintendents. It must have been winked at; nay, it must have been controlled by them. The existence of many local and restricted "surcharges," or corrections, proves that the loose paper of the one had been collated with that of the other computer; and thus the whole operation was conducted with a laxity of discipline which detracts enormously from its value.

But M. Lefort tells us, on page 996, that Prony was so jealous of the errors induced by transcription, that, when pressed by the Government to extract seven-place trigonometric tables from his extended ones, he preferred to proceed to their direct construction, rather than to incur the risk of the errors of copying. To me this appears an unintelligible motive; because, whether computed directly or not, the table must be copied in type, while the probability of exactitude is immensely in favour of the extended calculation. Lefort proceeds to say, "On no occasion did Prony or his collaborators say or give reason to think that there had been copying on the ruled sheets."

It is to be remarked, that Lefort does not advert at all to that very circumstance which gave occasion for his paper, namely, the

presentation to the Library of the Institute of a third copy of the great tables: yet he says, on page 995, that the collection of the leaves filled by interpolation was to form a double original; that such was the object, the only object of the operation, "Tel était le but, le but unique de l'operation." On what hypothesis, then, shall we explain the existence of this third copy "qui avait été laissé à Prony à titre de Minute." M. Prony could not have possessed it and been ignorant of the fact that at least one of the three was the result of transcription. Yet for these three quarters of a century the whole mathematical world has been led to believe, has believed, has acted, and has spoken in the belief that these "Grandes Tables" were constructed with every, the most scrupulous, attention to the requirements of exactitude.

Having made a most careful examination of the logarithmic part of the work—having performed the duty of a most conscientious witness in stating the facts as they appeared to him—M. Lefort has not ventured to sum up the evidence; but, speaking of the tables, concludes his paper with the words, "Je n'ai voulu aujourd'hui qu'en constater la valeur," leaving to others to form their own opinions of the exact value determined by his revelations.

He does, indeed, express a qualified commendation, for, at the foot of page 998, he says:—"The Cadastre Tables, like all human works, are then not perfect; they are so neither in their execution nor, perhaps, in the details of their conception; nevertheless, they much surpass, not only in extent, but also and above all in correctness, all the tables that have preceded them, and the more modern tables which have not been compared with them before publication."

Here M. Lefort has omitted to observe that he had been collating a manuscript calculation, in which there should have been no error, with printed books subject to all the chances of mistakes in reading and accidents in printing; he has surely also forgotten that these manuscript tables were so imperfect, and were known to their computers to be so imperfect as to be unfit for the verification of the last three places in the "Arithmetica Logarithmica," the very first work on denary logarithms, a work undertaken and completed by a private person, amid all the difficulties and round-aboutness of infant algebra. As to the details of the conception, he has told us that the orders of differences were extended to the sixth, that

the decimal places went to the twenty-sixth, with the admirable result of an exactitude not reaching beyond the twelfth place, where differences of even the third order barely appear. And above all, he has failed to perceive that what confidence we can now have in these prodigious piles of figures is derived from the labours of a single individual, whose zeal and perseverance led him to collate with the Cadastre table, the only two tables which preceded them, and to examine the divergences by help of calculations more trustworthy than either; that, in fact, the portion of the "Grande Tables" entitled to claim our confidence rests that claim on the joint labours of Adrian Vlacq and M. F. Lefort. The further *dictum*, that these tables are more exact than later ones which have not been compared with them, is supported by no evidence or argument, besides implying an obvious absurdity.

Concerning the rest of the Logarithmic Table, that belonging to numbers from 100,000 to 200,000 we have no information, because there existed no table for comparison, and our confidence must be founded exclusively on what we know of the principles of the method followed, of the fidelity of the execution, and of the candour of the statements. On these three heads, M. Lefort has placed before us information which it is not necessary for me to recapitulate.

The advantages of a uniform scale of numeration have been recognised in all ages. The ancient geometers adopted the basis 60, and their system has come down even to the present day. If the circumference of the earth be divided into 60 parts, each of these again into 60, and, once more, each of these subdivisions into 60 parts, we come almost exactly to the stadium; according to the same plan, the hour and the degree are each divided in sixtieths. It is the uniformity, the self-consistency of this system, which has so long preserved it. Twenty centuries ago, the sage of Syracuse placed before King Gelo the powers of the more convenient denary system; he showed how a few steps of a progression by myriads enabled him to express the number of grains of sand, not in the bay of Syracuse, not on the whole shores of Sicily, but that would be contained in a sphere having the moon's distance for its radius. The denary system has gradually gained supremacy in the languages

of all civilised nations, to the obliteration, in most of them, of every trace of any other system; 260 years ago, it was crowned by the invention of logarithms, which invention has rendered its exclusive use in every department of science only a question of time. Briggs computed a trigonometric table to each hundredth part of the degree; and near the end of the last century the still further improvement was proposed of dividing the quadrant itself into one hundred degrees, and each degree centesimally. Borda, about 1793, computed, to each minute of this division, a table, which was given in a compressed and inconvenient form by Callet, in his "Tables Portatives;" but an extension of this, at least so far as to each tenth second, was absolutely needed before the centesimal division could be used in the higher departments of geodesy and astronomy.

The French government, with a most enlightened regard for the interests of science, that is, for the interests of humanity, ordered the computation to be proceeded with, and entrusted the execution thereof to M. Prony. In confident expectation of the speedy appearance of this table, M. de La Place gave in his great treatise on Astronomy, the "Mécanique Celeste," the data and formulæ, according to the new division of the circle. The calculations have been finished, but have remained in the libraries of the Institute, and of the Observatory, inaccessible and useless to the general scientific public.

It would be difficult indeed to over-estimate the injury done to the progress of exact science by this calculation and this occlusion. accompanied, as they have been, by the pretence of exhaustive accuracy and unrivalled extent. There was enough of enterprise, enough of zeal, to have long ago completed the necessary work to seven places. Michael Taylor, in 1792, had finished a far more extensive work, the compilation of a seven-place table to each single second of the old division. But these existent and unpublished tables barred the way; for no private person would think of undertaking of new a work which had been already so well accomplished. Thus the most excellent and most laudable design of the French Government has been frustrated,—has been turned from a benefit to an injury.

Though sorely needed and urgently demanded, the new tables did not appear; and when expectation had been stretched to the

utmost, the English Government, in 1819, at the instance of Mr Davies Gilbert, proposed to defray one-half of the expense. The negociations led to no result. M. Lefort gives an extract from a note addressed by the celebrated astronomer Delambre to the English commissioner, apparently the farewell note. I transcribe the extract from page 999.

"Ces tables, non plus que celles de Briggs, ne serviront pas dans les cas usuels, mais seulement dans des cas extraordinaires. Comme celles de Briggs, elles seront la source où viendrous puiser tous ceux qui impriment les tables usuelles avec plus ou moin d'étendue. Elles serviront de point de comparaison pour tout ce qui a été fait ou se fera."

Whether shall we accept this magniloquent praise or the refusal to print the tables as the measure of their value? Even had these tables been all which they should have been,—all that was pretended for them,—the concluding sentence is preposterous. Is every calculation in all futurity to be tested by comparison with Prony? No! Even away from the revelations of M. Lefort, the independent original computer would not seek to dip his pitcher in the well at the Bureau de Cadastre, he only cares to fill his cup at the small everflowing spring of conscientious performance.

The tables of Prony cannot be printed without entire revision; in such a case to revise is to supersede, and therefore I call upon the whole body of cultivators of exact science to shake off this incubus, to hold these tables as non-existent, and to face manfully the problem of computing decimal Trigonometrical Tables of extent and precision sufficient for their pioneers, and therefore capable of supplying all the shorter and less precise tables needed for their more ordinary pursuits.

2. On the Elimination of α, β, γ, from the conditions of integrability of S. $u\alpha\delta\rho$, S. $u\beta\delta\rho$, S. $u\gamma\delta\rho$. By M. G. Plarr. Communicated by Professor Tait.

3. The Development of the Ova, and the Structure of the Ovary, in Man and other Mammals. By James Foulis, M.D. (Edin.) Communicated by Prof. Turner.

After an historical introduction, in the course of which the author gave an abstract of the important observations of Pflüger and Waldeyer, he proceeded to state his own observations on the development of the ova and structure of the ovary in calves, kittens, and the human female. The following general conclusions have been arrived at by the author in the course of his investigations:—

The corpuscles of the germ epithelium are derived by direct proliferation from those columnar corpuscles which invest the median side or surface of the Wolffian body, and which are continuous with the layer of columnar corpuscles that lines the pleuro-peritoneal cavity of the embryo in the early stages of development. The stroma of the ovary in the early stages of development is produced by a direct growth out from the interstitial tissue of the Wolffian body immediately beneath the germ epithelium on the median side of the Wolffian body.

The germ epithelial corpuscles proliferate by fission. In the human fœtal ovary of $7\frac{1}{2}$ months they measure $\frac{1}{2500}$ – $\frac{1}{2000}$ of an inch in their longest diameter, and about $\frac{1}{3500}$ of an inch in their shortest diameter. Each germ epithelial corpuscle is a nucleus surrounded by a thin film or investment of clear protoplasm. The nucleus of each germ epithelial corpuscle becomes the germinal vesicle of the mature ovum; and every germ epithelial corpuscle is potentially an ovum. In the act of becoming primordial ova, the nucleus of each germ epithelial corpuscle swells up into a spherical corpuscle with dark granular contents, within which is generally seen a nucleolus, and around which is produced clear homogenous protoplasm which subsequently forms the yelk of the ovum. Germ epithelial corpuscles are seen in all stages of development into primordial ova. In each primordial ovum the spherical germinal vesicle presents a sharply defined limiting membranous wall. Within the germinal vesicle is the nucleolus or germinal spot. All the ova in the ovary are derived from germ epithelial corpuscles.

In all parts of the ovary processes of vascular connective tissue stroma grow in, between and around certain of the germ epithelial

corpuscles, whereby the latter become more and more embedded in the stroma of the ovary. Germ epithelial corpuscles are being constantly produced on the surface of the ovary, to take the place of those already embedded in the stroma. The embedded corpuscles increase in number by division, and the nucleus of each swells up into a spherical germinal vesicle, around which is gradually produced the yelk of the ovum. In all parts of the young ovary under the germ epithelium, groups of germ epithelial corpuscles become embedded in meshes of the stroma. As each individual in the group swells up the nucleus or germinal vesicle of each becomes very distinct as a round or spherical body. From the swelling out of each germ epithelial corpuscle in the group, the whole group expands and becomes more or less spherical. Such groups of developing corpuscles are called egg clusters. Each egg cluster is enclosed in a mesh or capsule of vascular stroma of the ovary.

The stroma of the young ovary consists for the most part of fusiform connective tissue corpuscles and blood-vessels. The walls of the young blood-vessels in the young stroma consist of connective tissue corpuscles. These connective tissue corpuscles are direct offshoots from the ovarian stroma, and are found in contact with the yelk or protoplasm of each primordial ovum situated among the germ epithelial corpuscles on the surface of the ovary. Wherever we find primordial ova we see connective tissue corpuscles in contact with the yelk of each. In all parts of the ovary we find the nuclei of connective tissue corpuscles dividing. Sometimes these corpuscles are swollen out into round bodies containing three to four nuclei. In each egg cluster several of the included germ epithelial corpuscles are in a much farther advanced stage of development than their fellows. From the walls of the meshes enclosing the egg clusters, delicate processes of vascular connective tissue grow in, between, and around individual corpuscles in the egg clusters, and by a continued intergrowth of the young stroma in this manner each individual of the group becomes at last enclosed in a separate mesh or capsule. These last formed meshes are the Graafian follicles.

As a rule, each Graafian follicle is occupied by one young ovum. The protoplasm or yelk of each ovum is in close contact with the

wall of each Graafian follicle. In contact with the yelk of each young ovum, and indenting it, are connective tissue corpuscles, which form part of the wall of each Graafian follicle. In the formation of the membrana granulosa, these connective tissue corpuscles in the wall of the Graafian follicle, and in contact with the yelk of the contained ovum, increase in number by division, their nuclei swell out into little vesicles, and at last a perfect capsule of such corpuscles is produced round the ovum. This capsule is the membrana granulosa or follicular epithelium of the follicle. At first the membrana granulosa consists of a simple layer of cells lining the follicle. The individual corpuscles of the membrana granulosa measure about $\frac{1}{3000}$ inch. As the ovum becomes mature, the corpuscles of the membrana granulosa proliferate, and then many layers of small corpuscles are produced between the ovum and the follicular wall. The cells of the membrana granulosa are thus derived from the corpuscles of the connective tissue stroma, and not, as Waldeyer states, from the germ epithelial corpuscles. The follicular space is formed by a breaking down and probable solution of certain of the corpuscles of the thickened follicular epithelium in the middle parts of the same. The discus proligerus consists of follicular epithelial corpuscles, which are in contact with the zona pellucida of the ovum. The zona pellucida or vitelline membrane is formed by a hardening of the outer part of the yelk or protoplasm of the ovum, and is not, as Reichert, Pflüger, and Waldeyer stated, a product of the follicular epithelium. At birth the human ovary contains not less than 30,000 ova, few of which reach maturity.

In the human ovary at birth the germinal vesicles measure $\frac{1}{1800}-\frac{1}{1200}$ of an inch. Most of them are about the same size, and also present a sharply-defined membranous wall. In some germinal vesicles two or three germinal spots are seen. The tunica albujinea is the thickened stroma growing round the ovary. At the age of 2½ years all formation of ova from the germ epithelium has ceased.

Graafian follicles are not formed from tubular structures in the manner described by Pflüger, Spiegelberg, and Waldeyer. The appearances of tubular structures passing into the stroma of the ovary are produced by sections through furrows and depressions

between irregular prominences on the surface of the fœtal ovary. The irregularities of the surface of the fœtal ovary are produced by the expansion of egg clusters upwards under the germ epithelium. When the walls of furrows and depressions come in contact, egg clusters are formed by the embedding of germ epithelial corpuscles in that situation, just as in other situations. Egg clusters are formed in connection with the germ epithelium lining the furrows and depressions. Among the germ epithelium corpuscles lining the furrows, &c., we find large primordial ova, and corpuscles in all stages of development into the same, just as in other situations among the ordinary germ epithelial corpuscles.

At the age of six years the epithelium on the human ovary consists of very small flat hexagonal-shaped corpuscles, measuring $\frac{1}{2500}-\frac{1}{3200}$ of an inch. The corpuscles are seen dividing. This layer can be stripped off without difficulty. At the age of twelve the epithelium has little difference in appearance from the above, the small size of the epithelial corpuscles being remarkable. The epithelium is beautifully seen in old cats, and must be regarded as homologous with the peritoneal epithelium. In old cats the epithelium on the surface of the ovary consists of very small distinct cells, measuring from $\frac{1}{1800}$th to $\frac{1}{2000}$th inch, with granular oval nuclei.

4. Mathematical Notes. By Professor Tait.

(*Abstract.*)

(1.) On a singular Theorem given by Abel.

The theorem in question, in its simplest form, is

$$\pi f(x) = \int_0^x \frac{dy}{\sqrt{x-y}} \int_0^y \frac{f'(\xi)d\xi}{\sqrt{y-\xi}}.$$

Abel's proof of it involves the properties of the gamma-function, and requires that $f'(\xi)$ should be capable of development in powers of ξ. (*Œuvres*, I. 27.)

Independently of the interesting kinetic application for which it

was originally designed, this result is very curious, as suggesting a form of the *square root* of the operation of simple integration. In fact it gives

$$\left(\frac{d}{dx}\right)^{-\frac{1}{2}}(\quad) = \frac{1}{\sqrt{\pi}}\int_0^x \frac{(\quad)dy}{\sqrt{x-y}}.$$

Seeking to obtain an elementary proof of Abel's result, which should at the same time be applicable to any function, whether developable or not, I hit upon the simple expedient of inverting the order of the two integrations. We thus get the proof immediately in the form

$$\int_0^x\int_0^y \frac{dy f'(\xi)d\xi}{\sqrt{x-y}\sqrt{y-\xi}} = \int_0^x\int_\xi^x \frac{f'(\xi)d\xi dy}{\sqrt{x-y}\sqrt{y-\xi}}.$$

Now it is known (and a simple geometrical proof is easily given) that

$$\int_\xi^x \frac{dy}{\sqrt{x-y}\sqrt{y-\xi}} = \pi.$$

Hence the integral becomes at once

$$\pi\left[f(x)-f(0)\right].$$

Numerous extensions and applications of the theorem are given.

As one example of these extensions the following, which assigns an expression for $\left(\int_0^x\right)^{\frac{1}{n}}$, may here be given—

$$\int_0^{x_1}\frac{dx_2}{(x_1-x_2)^{\frac{n-1}{n}}}\int_0^{x_2}\frac{dx_3}{(x_2-x_3)^{\frac{n-1}{n}}}\cdots\cdots\int_0^{x_n}\frac{f'(\xi)d\xi}{(x_n-\xi)^{\frac{n-1}{n}}}$$

$$= E_1E_2\ldots\ldots E_{n-1}\left[f(x_1)-f(0)\right].$$

Here

$$E_r = \int_0^1 \frac{de}{(1-e)^{\frac{n-1}{n}}\, e^{\frac{n-r}{n}}},$$

and therefore

$$E_1E_2\ldots E_{n-1} = \frac{\Gamma\left(\frac{1}{n}\right)\Gamma\left(\frac{1}{n}\right)}{\Gamma\left(\frac{2}{n}\right)} \cdot \frac{\Gamma\left(\frac{1}{n}\right)\Gamma\left(\frac{2}{n}\right)}{\Gamma\left(\frac{3}{n}\right)} \cdots \frac{\Gamma\left(\frac{1}{n}\right)\Gamma\left(\frac{n-1}{n}\right)}{\Gamma(1)} = \left(\Gamma\left(\frac{1}{n}\right)\right)^n.$$

Hence

$$\left(\frac{d}{dx}\right)^{-\frac{1}{n}}(\quad) = \frac{1}{\Gamma\left(\frac{1}{n}\right)}\int_0^x \frac{(\quad)dy}{(x-y)^{\frac{n-1}{n}}}.$$

The theorem given by Abel is easily seen to be the particular case of this when $n = 2$, for then

$$\left(\Gamma\left(\frac{1}{2}\right)\right)^2 = \pi.$$

Another form of the above multiple integral is easily seen to be

$$x_1\int_0^1 \frac{e_1^{\frac{n-1}{n}}de_1}{(1-e_1)^{\frac{n-1}{n}}}\int_0^1 \frac{e_2^{\frac{n-2}{n}}de_2}{(1-e_2)^{\frac{n-1}{n}}} \cdots \int_0^1 \frac{f'(e_1e_2\ldots e_nx_1)de_n}{(1-e_n)^{\frac{n-1}{n}}};$$

and curious expressions for $\left(\frac{d}{dx}\right)^{-\frac{2}{n}}$ (when n is even) may be obtained by evaluating the integral

$$\int_0^{x_1} \frac{dx_2}{(x_1-x_2)^{\frac{2(n-1)}{mn}}}\int_0^{x_2} \frac{dx_3}{(x_2-x_3)^{\frac{2(m-1)(n-1)}{mn}}} \cdots\cdots$$

$$\cdots\cdots\int_0^{x_{n-1}} \frac{dx_n}{(x_{n-1}-x_n)^{\frac{2(n-1)}{mn}}}\int_0^{x_n} \frac{f'(\xi)d\xi}{(x_n-\xi)^{\frac{2(m-1)(n-1)}{mn}}}$$

where m is any real quantity whatever.

Other instances of the use of this process were adduced, but those just given are sufficient for an abstract like the present.

(2.) On the Equipotential Surfaces for a Straight Wire.

Some results given in Vol. I. of *Thomson and Tait's Natural Philosophy* may be much more simply obtained by calculating the potential of a wire rather than its attraction. That potential is easily found as

$$\rho \log. \frac{r_1 + r_2 + c}{r_1 + r_2 - c},$$

where c is the length of the wire, ρ its line density, r_1 and r_2 the distances of its ends from the point at which the potential is to be found.

In this form we see at a glance that the equipotential surfaces are prolate ellipsoids of revolution with common foci at the ends of the wire.

The method is extended to polygons plane or gauche, closed or unclosed.

(3.) On a Fundamental Principle in Statics.

The principle that, while additional constraints cannot disturb equilibrium, unnecessary constraints may be removed without disturbing equilibrium, is of very great use in the statics of fluids and of elastic and flexible bodies. But it seems not to have been made use of to the extent its importance deserves.

My attention was recalled to it when attempting to compare the shares taken by gravity and cohesion in resisting the tendency of the so-called centrifugal force to split a planet. The problem which first proposed itself was to determine the gravitation attraction of one-half of a uniform sphere upon the other.

The sextuple integral which a direct solution of this problem would require may be entirely dispensed with, and its place supplied by a simple single integral, if we imagine a thin film of the solid on each side of a diametral plane to be converted (without change of bulk or density) into an incompressible liquid.

Or we may commence with a sphere of homogeneous incompressible liquid. If a be its radius, ρ its density, it is easily shown that the whole pressure normal to any diametral plane—which is of course the attraction of the hemispheres on one another—is

$$\frac{1}{3}\pi^2\rho^2a^4.$$

If each hemisphere were collected at its centre of inertia the attraction would be $\left(\frac{4}{3}\right)^3$ times as great. The centrifugal force tending to split the planet across a diametral plane through the axis (it is easily shown to be greater per unit of area on a diametral than on any other plane) is

$$\frac{1}{4}\pi\rho\omega^2a^4,$$

where ω is the angular velocity of rotation. The ratio of these is

$$\frac{\frac{4}{3}\pi\rho a}{a\omega^2}$$

or the ratio of gravity to centrifugal force at any point on the equator. Hence, so far as gravity is concerned, the earth would split across a meridian if it were to revolve more than seventeen times faster than it does.

It is known that, if the earth revolved seventeen times faster than it does, centrifugal force would just balance gravity at the equator. The relation of this fact to the above statement depends upon the geometrical proposition that the volume of a very small slice from the surface of a sphere is half the product of its thickness by the area of its base.

And cohesion would not sensibly alter this state of things; for, assuming the earth's diameter to be 8000 miles, its mean density 5·5, and the weight of a cubic foot of water at the surface 63 lbs., while the average tensile strength of its materials is taken as 500 lbs. weight per square inch, the cohesion between two hemispheres is shown to be only $\frac{1}{25{,}410}$th part of their gravitation attraction.

Even if we made the extreme assumption that the tensile strength is (throughout) that of steel, cohesion would in the case of the earth be only about $\frac{1}{100}$th of gravitation attraction between hemispheres.

As a consequence, a planet of the earth's mean density and the above assumed tensile strength is held together as much by cohesion as by gravitation if its radius is $\frac{1}{\sqrt{25,410}}$th of that of the earth, or about 25 miles. If of steel's tenacity it would have a radius of about 409 miles.

Monday, 4th January 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The President exhibited and described his Tide Calculating Machine, also his Improved Tide-Gauge; he also described certain Capillary Phenomena, with Experiments.

The following Gentlemen were elected Fellows of the Society:—

C. H. MILLAR, Esq.
JOHN MILROY, Esq.
Professor DANIEL WILSON, Toronto.
ANDERSON KIRKWOOD, LL.D.
Dr LUDWIK BERNSTEIN.
DANIEL G. ELLIOT, Esq., New York.
ROBERT GRAY, Esq.
WILLIAM CRAIG, M.D., F.R C.S.E.

Monday, 18th January 1875.

The HON. LORD NEAVES, Vice-President, in the Chair.

The following Obituary Notices of Deceased Fellows of the Society were read:—

1. Biographical Notice of Lord Colonsay. By the Hon. Lord Neaves.

By the death of Lord COLONSAY, this Society has lost a member of great distinction, and well worthy of being held in respectful remembrance. He was a man of great vigour of mind, and with powers and qualities which would have earned for him a high place

in science or in literature if they had been turned in either of these directions; but it was his lot and his choice to follow a professional career, in which, as will afterwards be seen, he came to attain all the varied honours which the practice of the law is able to confer.

Duncan M'Neill was the second son of John M'Neill, proprietor of the islands of Colonsay and Oronsay, and of the estate of Ardlussa in Jura, and was born in Oronsay on the 20th of August 1793. He was not educated at any school, but received private tuition at home along with his brothers, until he repaired to the University of St Andrews, along with his immediately younger brother, now Sir John M'Neill.

He used in after life to tell of an incident that occurred to the party when his father and the two boys passed a night in Glasgow on their way from the Highlands to St Andrews, and it was certainly one well calculated to make a permanent impression on a vigorous and appreciative mind. While he was walking in the morning, near the post-office, a mail-coach arrived, from the roof of which the guard announced to an assembled multitude the news of the victory of Trafalgar, which occurred on 21st October 1805. The intelligence, of course, was received with tumultuous cheering, after which, one of the crowd proposed three cheers for Nelson, but when the guard in a loud and sad whisper said "that Nelson was killed," they all instantly dispersed in solemn silence, and left the streets empty.

Duncan M'Neill was twelve years old when he went to St Andrews, which was not at that time an unusual age for college intrants. He and his brother were boarded with Dr James Hunter, professor of Logic, for whom and for his family M'Neill always entertained a strong feeling of attachment and regard. He became a diligent student and a good classical scholar, but was still more distinguished in mathematics, for which he had a remarkable aptitude.

After three years spent at St Andrews he came to Edinburgh, and attended college here for some sessions. As usual with young men of intellectual power, he applied himself diligently to Logic and Metaphysics, for the latter of which, undoubtedly, Dr Thomas Brown, whom he attended, was calculated to inspire a strong taste, though Brown himself was not a profound or perhaps even a sound metaphysician. His lectures, however, were pleasing and attrac-

tive, and had often the effect of leading his hearers to larger and deeper views than their teacher entertained.

It has been stated in a very able and kindly notice of Lord Colonsay, ascribed to Mr Campbell Smith, that about this period, or shortly afterwards, M'Neill formed the intention of publishing the philosophical works of David Hume, of which at that time no good collective edition existed. I am not able to confirm this statement by my own testimony, but I know well that he was always a great admirer of our greatest Scottish philosopher. He was not likely to be led away into those sceptical speculations which Hume propounded from his attempting to introduce metaphysics into a region which lies above their reach, nor was he likely to follow Hume in the perverse preference which he seemed to feel for French literature over English, and which may be traced partly to the influence of prejudice, and partly to a feeling that he was less appreciated in England than on the other side of the channel. But in other respects the mind and style of Hume were well calculated to please and influence M'Neill in matters of reasoning and of philosophy. The simplicity and brevity with which he wrote, the caution and moderation with which he stated his opinions, and the calmness with which he dealt with his adversaries, were all congenial to the tastes and feelings with which M'Neill was wont to approach questions of evidence and reasoning. It is not to be supposed that he was destitute of feelings and energies to which Hume was a stranger. His Highland or Island blood was more fervid than any that circulated in Hume's veins, and his early life and athletic frame were a strong contrast to the indolent and somewhat obese form of the philosopher of the Merse.

With a view to a professional life, M'Neill entered on an apprenticeship in the chambers of Mr Michael Linning, W.S., and discharged with regularity and diligence the duties that there devolved upon him.

I am not sure whether it was originally intended by his friends that M'Neill should come to the bar, or whether the remarkable talents which he soon displayed led to his adopting that profession instead of that of a writer to the Signet, to which his initiation at Mr Linning's would naturally have led. But it cannot be doubted that the time passed and the instruction received by him

in a writer's chambers, as well as the friendships which he there formed, were eminently useful to him at the bar.

Lord Colonsay passed advocate in 1816, and amidst a great number of eminent contemporaries and rivals he soon became distinguished in his profession. He devoted himself with special diligence to the study of criminal law, which he thoroughly mastered, and made himself so formidable as an opponent in defending prisoners that the Crown authorities saw the advantage of securing his services on their side, and in 1820 he was made an Advocate-depute by Lord Meadowbank, then Lord Advocate.

In 1822 he was appointed Sheriff of Perth, in room of Lord Medwyn, promoted to the Bench. He continued in that office with great efficiency and usefulness down to 1834, when he became Solicitor-General under Sir Robert Peel's administration. That ministry retained office for only a few months; but when they returned to power in 1841, he was again made Solicitor-General. In October 1842 Sir William Rae, then Lord-Advocate, died, and M'Neill succeeded him in that office. In 1843 he was elected Dean of the Faculty of Advocates, and became Member of Parliament for Argyllshire, holding that position from 1843 to 1851, when he was promoted to the Bench by the Whig Ministry at the same time with Lord Rutherfurd. In 1852 he was made Lord Justice-General and President of the Court of Session. After serving in that high position for fifteen years he was created Baron Colonsay in 1867, when he retired from the bench.

Thus it is that Lord Colonsay passed through all the grades and honours of his profession, from that of a simple advocate to the Presidency of the Court. We do not know if this is unprecedented, but it certainly has rarely happened that a member of the bar has become successively, as Lord Colonsay did, a Depute-Advocate, a Sheriff, Solicitor-General, Lord Advocate, Dean of Faculty, an ordinary Judge, and finally Lord Justice-General and Lord President. The varied functions and wide experience which these successive positions involved, could not fail to qualify him in the highest degree for the discharge of all his duties, and above all, of those which ultimately devolved upon him when placed at the head of legal administration of Scotland. Every professional man knows that the inferior grades of legal preferment are eminently

conducive to furnish the necessary knowledge and practice required for higher positions. It cannot be doubted that great experience as an advocate at the bar is of the highest use in discharging the functions of the bench. Under some national systems, Judges have been chosen who had not practised as advocates, but they would certainly not possess in that way the intelligence and penetration which an experienced barrister acquires, and which must enable him when on the bench to weigh the evidence, to detect the truth, and to see quickly through the fallacies and disguises to which litigants are apt to resort. In another way the exercise of the inferior jurisdiction of Sheriff brings the holder of office into closer contact with country matters, and with local and customary considerations, which will serve him in good stead when as a Judge he comes to sit in review upon County-Court procedure.

Lord Colonsay was every way qualified for the profession which he adopted, and for the offices which he held. His talents, which were great, were eminently of a forensic and still more of a judicial character. His logical acumen was severe and unerring. He possessed also, though he never exercised it unnecessarily, a power of vivid and impressive eloquence, in which he was equalled by few and surpassed by none. He was a most able criminal advocate, and indisputably the greatest criminal lawyer of his day. His natural powers were aided and improved by patient and laborious study as a young man, and by the most conscientious and careful discharge of duty in all matters that came before him, whether at the bar or on the bench. Those who had the advantage of meeting him in consultation as an advocate, will bear testimony to the thorough mastery which he always attained of his client's case, and to the sagacious and skilful perception which he also acquired of the probable case of his opponent. In consultation he was entirely free from the petty selfishness that has sometimes been laid to the charge of seniors in bottling up their best views for their own use. Whatever point he thought advantageous to the case was always fully communicated and explained to his juniors.

In the practice of his profession as an advocate Lord Colonsay had some advantages not equally enjoyed by some of his brethren. The subjects with which an advocate has to deal are so various,

and often so special and technical, that it is impossible for any man to have a thorough and independent knowledge of all. The advocate has what may be called a *nisi prius* faculty of learning, on short notice, what he knew nothing of before, and then forgetting it when the occasion is over, in order to make room for new acquisitions equally temporary and transient. His great art consists in knowing where information is to be found, and making the appropriate use of it for his immediate purpose. In an extensive practice an advocate is thus brought in contact with questions of the most dissimilar kind—commerce, agriculture, engineering, chemistry, and many others, arising out of multifarious patents or contracts that become the subject of litigation. I once was able to illustrate this somewhat oddly to a man who knew many subjects and wrote many books. The late Mr MacCulloch, the political economist, once asked me in company whether his "Commercial Dictionary," which is a very useful book, was ever founded on or quoted in our courts of law? I answered rather abruptly, "Never; the name of it is never heard." He appeared disappointed at this, and I then added, "But very often a case comes in to us at night to prepare for next day, on a subject we know nothing about—general or particular average, foreign exchanges, or the like—upon which we go to our shelves and take down a Commercial dictionary, which enables us to appear at the bar when wanted next day with an amount of information that astonishes even our own clients. But we never mention the book from which the information is got." This statement seemed completely to re-establish the self-complacency of the sensitive author.

I would say here that Lord Colonsay, from his scientific tastes and tendencies, was more fully and accurately grounded in many of these questions than the most of his brethren. And this could not fail both to lighten his labours and to give confidence to his views.

As a judge, his judgments were models of clearness and brevity, and were always remarkable for an anxiety to maintain the great landmarks of legal principle. If he had a fault, it was one which, I think, in judicial business, "leans to virtue's side." When he felt that he could not be bold he was apt to be very cautious, and certainly was ever anxious not to decide any case but the one

that was immediately before the Court, leaving other cases to be determined at their own time, and after fully hearing the arguments that were specially directed to discuss them; and I am much inclined to think that it is better always to decide nothing but the actual question raised, or necessary to be decided, as no collateral point can in general receive the mature treatment and consideration that it deserves. His perfect command of temper, his great patience in listening, and his uniform courtesy on the bench earned for him the respect and gratitude of the whole bar, and added greatly to the weight and authority of his judgments.

We should not fully do justice to Lord Colonsay's merits if we did not notice and acknowledge the important benefits which the country has derived from his legislative exertions. At an early period, I believe, we may say that the great improvements made on criminal procedure in Scotland, by an Act in the ninth year of George IV., emanated from Lord Colonsay, though Sir William Rae was at that time Lord Advocate. The older forms of criminal process in Scotland, whatever may have been said to the contrary, were highly, and perhaps unduly, favourable to accused persons—in this respect, at least, that many formal objections to the designation and citation of witnesses and otherwise could be kept back till after a jury was empanelled, and could then be brought forward so as to frustrate the proceedings, while at the same time the accused could not be tried again in consequence of having "tholed an assize." This state of things, of which no one could make a better use than Lord Colonsay when defending prisoners, was abolished; so that all formal objections must now be brought forward at once before empanelling a jury, and thus, even if they prove fatal, the accused can be tried again on a new indictment.

When in Parliament as Lord Advocate, Lord Colonsay passed, or assisted in passing, many useful measures; but perhaps the most conspicuous of these is the Poor Law Amendment Act—a wise and beneficent measure, which has gone far to solve the great social difficulty of relieving pauperism without paralysing industry or oppressing ratepayers, many of whom must always be nearly as poor as the objects who obtain relief.

In all matters of legal reform, Lord Colonsay's services have always been at the command of his country, and though unosten-

tatiously performed, have been thoroughly appreciated by those who had the means of knowing and the power of judging.

Of the debt which we owed to Lord Colonsay after he took his seat in the House of Lords, it is unnecessary to speak.

I may here advert to a part of Lord Colonsay's life which possesses much interest, and is calculated to throw a strong light upon his character. Some time after the death of his father he became by a family arrangement the proprietor of Colonsay and Oronsay, which he retained till a comparatively recent period. In consequence of the advanced age of his father, these estates had not latterly been administered with as much energy and enterprise as the times demanded. They were all in the hands of the proprietor, except some small possessions held by a number of crofters and cotters. When Lord Colonsay acquired the property, he applied himself vigorously to putting it into perfect order. Besides visiting it during the vacations of the Court, he personally directed the whole improvements which were made upon it, and for that purpose transmitted, in the midst of the labours of his profession, minute directions weekly to his managers on the spot, and received their detailed reports of everything that was doing. In a few years he had the islands put into a most satisfactory state for being let out in separate farms of suitable size. The stock on the farms was every way improved. He encouraged and liberally aided emigration, and did so with singular delicacy, so as to spare the feelings and not impair the means of the emigrants. Excellent farm houses and offices were built, roads formed, and harbours improved at a very great expense, and at last he succeeded in lightening his own labours and establishing in the islands respectable tenants whose occupations gradually increased in value. He also succeeded in getting Colonsay detached from Jura and made a separate parish; and having improved the church that had been there in use, and built a comfortable manse and good school, he settled a liberal endowment on the minister, and thus gave the people on the island the advantages of a regular and efficient ministry, and two good parish schools. It may gladden our friend Professor Blackie's heart to hear that he retained his Gaelic in perfection to the last, and was thus enabled to exercise an influence that might otherwise have been lost.

I shall add only a few more words as to his personal life. He was never married, but his younger brother Archibald, with his wife and family, were for many years domesticated with him, and when his brother died, the widow and surviving children remained with him as before, and ultimately shared in a large portion of his means. He was a most affectionate relative, and a very firm friend. He never forgot a kindness received, and had particular pleasure in repaying, when it came to be in his power, any proofs of friendship which he had received in the earlier period of his career, when encouragement and assistance were calculated to be of such value. He was a man of great goodness of temper, and of inflexible justice in all his dealings. His estate of Colonsay he had disposed of before his death to his brother Sir John M'Neill, under a family arrangement.

For a considerable part of his life Lord Colonsay laboured under some weakness in the chest and breathing tubes, and latterly a tendency to bronchitis was perceptible. We believe it was to this malady that he fell a victim. He was only ill for a short time, and at the age of eighty it was not wonderful that he was unable to resist the influence of a disease so dangerous in general to those advanced in life.

2. Biographical Notice of Cosmo Innes. By the Hon. Lord Neaves.

We have lost another eminent member of our Society in Mr Cosmo Innes, of whom I shall venture to give a short account. I do not think it necessary to make it long, and this for various reasons. Mr Innes's labours were more nearly akin to the studies of another Society which meets under the same roof with ourselves, and within that body, I believe, tributes have been paid to his memory far more intelligent and more worthy of his reputation than any I could venture to offer. The general features of his career, also, are so well and widely known, and have been recalled to our recollection of late in such various ways, that any detailed narrative would be superfluous. My endeavour now, therefore, will mainly be not to pay homage to his antiquarian attainments, which are

indisputable, nor to the works of interest and utility which have proceeded from his industry, and which are never likely to be forgotten or to remain unappreciated, but to bear my testimony to his general accomplishments, and to his high personal character. Of these I claim a right to speak, from an unbroken friendship of upwards of sixty years, varied by much vicissitude of events, much community of favourite studies, constant professional or official intercourse, and domestic familiarity of the warmest and most pleasing kind.

Mr Innes was born on 9th September 1798. He was educated at the High School of Edinburgh, and at the University of Glasgow, from which last he proceeded on a Snell exhibition to Balliol College, Oxford.

It is well known, and necessary to be remembered, that the position of Mr Innes's family while he was yet a young man, came to be greatly affected by a misfortune that befell his father. Mr Innes, senior, who was a Writer to the Signet, was induced to give up business, and take a long lease of the estate of Durris, in Kincardineshire, upon which he expended great sums of money in improvements. But when the time approached for reaping the benefits of these, the lease was set aside, and the estate carried off by an heir of entail, leaving Mr Innes, senior, with a very slender equivalent for all the time and money he had thus expended.

One good thing resulted from this calamity. It brought out the native courage and vigour of Mr Cosmo Innes's character, and forced him to grapple manfully with his difficulties. His motto in such circumstances might well have been *Tu ne cede malis; sed contra audentior ito.* He never sat down with a listless look or a desponding heart, but turned to the first opening he could find that promised an escape from trouble. And here, as she generally does, Fortune favoured the brave, and gave our friend both a stimulus and an opportunity for exertion that might not otherwise have existed.

Another advantage that arose from the strong interest felt by all who saw his position, was that it excited the sympathy and attention of many friends of great influence and value. Much the most important of these, and one who greatly moulded and affected his future career, was Mr Thomas Thomson, whose acquaintance he

formed in the year 1824, and with whose labours he became, for a long period, substantially identified.

Thomas Thomson was one of the most able and learned antiquaries and "Record Lawyers" that Scotland has produced, and he would probably have been recognised as the greatest among them, if his efficiency had not been marred or impaired by some defects of character and peculiarities of taste which interfered greatly with his practical powers. His fastidiousness, his aversion to hasty or ill considered opinions, and his general tendency to procrastination, led him to allow duties to stand over that should have been instantly and resolutely performed. As a member of the "Record Commission" he became busily occupied in the arrangement of the Ancient Records and Muniments of Scotland, and the publication of the old Acts of Parliament of the country came to rank as the "magnum opus" of his life. At the time when Mr Innes became acquainted with him, he was completing, or had completed, the eleventh volume of that collection, but the first volume of it had not been begun, being the portion of the work attended with the greatest difficulty, involved in the deepest obscurity, and for which new materials were daily coming to light from sources hitherto undiscovered.

The character of Mr Thomson, and his eventful history, full of varied incidents, some of a most pleasing, and some of a most painful kind, are exhibited in the interesting Memoir of him written after his death by Mr Innes, at the request of Mr James Craig. The latter years of Mr Thomson's life were obscured by no ordinary gloom of misfortune. In his administration as a "Record Commissioner," and as "Depute Clerk-Register," his accounts were allowed to run into great arrear and confusion, and attention came at last to be called to them by the officials connected with the financial departments of the Government. There had, undoubtedly, been great neglect, and considerable disregard of the proper limits of expenditure, which it was found wholly impossible to justify, but which, I am satisfied, would all have been put right by Mr Thomson and his many friends, if time had been allowed. But some of the officials concerned, particularly the men of mere routine, were too peremptory, and too punctilious, to look to anything but purely arithmetical considerations, and that, perhaps, took place which is

not unfrequently observed, that injustice is done to a man by his political friends for fear of their being supposed to show him undue favour by protecting him from attack. However this may be, a step was taken which, in the opinion of many, was greatly to be deprecated.

A *criminal* charge was preferred against Mr Thomson for *defalcation* in his accounts, and it became necessary for him to appear for examination before the sheriff under that charge. At this time a change of government took place, and it happened that, as an official under the new crown authorities, I was entrusted with the duty of conducting Mr Thomson's examination. It was carried out with every degree of fulness and particularity, and I had much satisfaction in being able to report to my constituents that there were no grounds for a *criminal* charge. Mr Thomson had been guilty of laxity and carelessness, he had sometimes mistaken and exceeded his powers of expenditure, and he had ventured upon disbursements for what he considered to be important objects not authorised by the strict letter of his instructions. But there was no trace of anything corrupt or fraudulent, and the application of the *criminal* law to his case appeared to mea harsh and inappropriate proceeding. These views were adopted by the crown counsel of the day, and Mr Thomson was liberated from any responsibility beyond the civil consequences of his pecuniary errors. It was impossible, however, that such occurrences, overtaking a man of Mr Thomson's high position, unblemished character, proud feelings, and eminent public services, should not be overwhelming, particularly at the advanced period of life which he had reached. The whole colour of his existence was thus changed; he had lost his office of "Clerk Register," and although he retained that of "Clerk of Session," the salary attached to it was appropriated to the discharge of his debts. "It was intimated to him at this time that another person was to be employed to complete the first volume of the Acts of Parliament." This is the language in which the occurrence is mentioned in the Memoir of his life. Mr Innes was the person so employed, and nothing could well be conceived more painfully interesting on both sides than the relation that came thus to exist between the pupil and his old master. Mr Thomson must

have felt deeply the blow that thus deprived him of the opportunity of completing the crowning act of his long labours.

"He never again entered the Register House;" and Mr Innes adds, "that although he was generously communicative on every other point, where his assistance or advice was desired, he told me soon after I had been employed to complete the first volume of his great work, that *it must be a forbidden subject between us.*"

In 1844 Mr Innes finished the first volume thus handed over to him, and did so in a manner which gained, I believe, universal approbation. I do not say that it was done as well as Mr Thomson at one time *could* have done it, but I am sure that it was done as well as Mr Thomson could *then* have done it, or rather, that the difference lay between its being done well by Mr Innes and its not being done at all.

The extinction that was thus given to Mr Thomson's efficiency in his peculiar department, for such was truly the result of these events, left Mr Innes as almost the only man in the field to whom either the public or individuals could resort for advice and assistance in matters of this kind, and he thus became one of our highest authorities on the subject of general or family antiquities.

It cannot be said, I think, that Mr Innes was ever successful as an advocate. He did not possess in a sufficient degree either what has been scornfully called the power "to make the worse appear the better reason," or which, I think, is its more correct description, the peculiar faculty on a properl deebateable question, to bring forward the fair and legitimate considerations that are to be weighed on either side. But he held successively important official appointments, that of Advocate-Depute, Sheriff, and principal Clerk of Session, the duties of which he discharged with adequate diligence. He was latterly appointed to the chair of Universal History in the University of Edinburgh, which was highly congenial to his general pursuits, and in which, I believe, he endeared himself to his students by his uniform accessibility and kindness, and by the valuable aid which he afforded them in their studies.

I have disclaimed any intention here of attempting to enumerate or estimate the different works of an historical or antiquarian kind which Mr Innes produced. I shall merely advert to his "Scotland in the Middle Ages," published in 1863, and his "Sketches of early

Scottish History," published in 1861, both of which are well known and are peculiar. Besides these, I may add in the words of Mr David Laing, which I am allowed to borrow, that "his labours in editing numerous volumes of ancient chartularies for the Bannatyne, Maitland, and Spalding Clubs, more especially those of Melrose, Moray, Holyrood, Dunfermline, Glasgow, and Kelso, as well as works connected with the public records of Scotland, will always be gratefully remembered." One of the works undertaken by him was the "Origines Parochiales of Scotland," which, if it could have been finished as it was begun, would have been a great and valuable work; but the difficulties in its execution proved to be far greater than had been calculated, and it remained at last in an unfinished state, which necessarily diminished its utility and importance.

I have always understood that the manner in which Mr Innes prepared the official works which he was able personally to accomplish, was much admired and approved of by the best judges both in this country and abroad, and in particular I have heard that M. Guizot, no mean critic, to whom he was personally known, always spoke highly of their merits. Partly on business exigencies, and partly as a form of relaxation, Mr Innes was latterly in the habit of visiting Paris in time of vacation, and greatly enjoyed the advantages of good Parisian society, as well as the opportunity thus afforded him of access to the French archives and other objects connected with mediæval history and antiquities. I may here observe that Mr Innes, among other accomplishments, had a very decided talent for letter writing, and that when he was abroad the accounts thus conveyed to confidential friends of what he had seen and felt on his travels, were a source of great interest and delight.

In Mr Innes's character—let me rather say within his bodily frame—two very different aspects of human power were to be seen. In the one we had a strong and athletic man, passionately fond of the country and country scenes, particularly those of this "Land of the Mountain and the Flood," the "Land of our Sires," excelling in all country sports, fishing, shooting, riding, coursing, and enjoying a pleasing though always a temperate repose from these exertions in some friendly or social meeting; while, in the other, we saw a man turned into a monk, busy among libraries and state records all day, and poring with double magnifiers and strong lamps till long

after midnight, deciphering old and almost illegible manuscripts, and trying at once to master their character and make sense of their contents. These very different capacities and functions existed harmoniously together in the same individual, and instead of interfering with each other, communicated, perhaps, a mutual zest, and enabled the change to be pleasantly or at least contentedly acquiesced in. The versatility thus existing and kept up fitted him for a very varied and interesting range of social acquaintances, and of these he was always glad to avail himself in moderation. Nor was any one a more agreeable companion. His perfect good humour and good temper, his strong affection for his family and for his old friends, his never-failing courtesy, which arose from and indicated the chivalrous feeling that was at the foundation of his character, his utter absence of envy, jealousy, presumption, or self-conceit; and his sympathy with all innocent and gentlemanly relaxation and even merriment, endeared him to a very extensive and attached circle, and made his home the centre of much attraction and the scene of much social enjoyment. To these enjoyments his surviving friends still look back with unmixed pleasure and tender regard.

His literary productions, apart from those which appeared in an official form, show the same diversity of character to which we have already alluded. As specimens of these I may mention two excellent but very different papers, which a careless reader would scarcely conceive to have proceeded from the same mind: the one of these, a contribution to the "Quarterly Review" in 1843, upon the Ecclesiastical Antiquities of Scotland, and the other a paper inserted in the "North British Review" in 1864, on the Country Life of England. Each of these is well deserving of perusal, and the last mentioned is particularly interesting, as having first introduced into notice the achievements and writings of Charles St John, the well-known lover of sport, with whose tastes and habits those of Mr Innes were in full accordance, so far as circumstances would permit of their free indulgence.

Mr Innes's love for literature was strong and diversified. He was a fair Greek and Latin scholar. I hesitate to call him a *good* Greek scholar, as my old friend Archdeacon Williams denied that title to any one who did not know every good Greek author from Homer to Agathias. He was sufficiently at home in French and Italian

to serve all the purposes which he had in view. But I think the books that he most loved were those that gratified best that chivalrous feeling that lay so deep in his heart. I remember as if it were yesterday hearing him read, fifty years ago, in an Italian society to which we belonged, the concluding character of Sir Lancelot, given in Malory's translation of the Morte d' Arthur, which runs in these striking terms:—"And now, I dare say, that, Sir Lancelot, there thou lyest; thou wert never matched of none earthly knight's hands. And thou wert the curteist knight that ever bare shielde. And thou wert the truest friende to thy lover that ever bestrode horse. And thou wert the truest lover of a sinful man that ever loved woman. And thou wert the kindest man that ever stroke with swerde. And thou wert the goodliest person that ever came amonge prece (press) of knights. And thou were the meekest man and the gentillest that ever eate in hal among ladies. And thou were the sternest knight to thy mortale foe that ever put spere in the rest!"

Mr Innes read these words with the greatest effect, but in that peculiar tone for which I think his reading was remarkable. He never read rhetorically, or in a declamatory style, but with rather a cold and dry manner, which, however, had the strange effect of leaving on his hearers a deep impression of his earnestness, and a thorough belief in what he said. It was impossible so to hear him without feeling convinced, as I then and ever was, that his own character involved in it many of those noble traits that the romancer described as forming the bright side of his hero.

Mr Innes's death was sudden, and took place at a distance from home, but it was calm and painless, and he was attended at the time by his wife and his only unmarried daughter. It is right to mention that in the later years of his life he enjoyed the advantage of a considerable accession of fortune, which came to Mrs Innes, and which placed them in comparative affluence. At the time he was taken away, his daughter was engaged under very happy auspices to the gentleman who has since become her husband, so that his departure took place amid circumstances that brought many consolations, and left little more in life to be desired.

3. Biographical Notice of Francis Deas. By the Hon. Lord Neaves.

Another loss to our Society which we have to record and to deplore at this time, arises by the death of Mr Francis Deas. This loss forms a striking contrast to that of either of the members of whom I have already spoken. They retired from the scene not prematurely, but full of years and well-deserved honours, having attained or approached the longer limit to which human life in normal circumstances is considered to extend; they had played out their parts, and, as having done so, were entitled to their dismissal amidst the plaudits of those who had witnessed and benefited by their labours. Mr Deas, on the other hand, was cut off, first by failing health, and ultimately by death, before he had attained the meridian of life, or could carry out into execution the capacities which, under a more favourable fate, would assuredly have earned him high distinction.

Francis Deas, the eldest son of the Hon. Lord Deas, was born at Edinburgh on the 1st July 1839. He went through the usual curriculum of the Edinburgh Academy, which he quitted in July 1856, having held a good place in all his classes, and having gained in 1855 the Ferguson medal, and in 1856 the Mitchell medal, both of them for proficiency in mathematics. He then went through the usual course of study at the Edinburgh University, taking prizes in almost all his classes—mathematics, logic and metaphysics, civil law, Scots law, rhetoric, and belles lettres, and natural philosophy; but he did not confine his studies to the usual routine. He was a zealous student with Professor Balfour for two or more sessions in botany, and accompanied him in his pedestrian excursions. He attended Dr Stevenson Macadam for practical chemistry, Professor Allman for natural history, and Dr Maclagan for medical jurisprudence. He continued in after life to keep up an intimacy with many of the Professors whose instructions he had thus received.

In 1859, before he was twenty, he went to Berlin, principally in order to perfect himself in speaking German, with which he was otherwise well acquainted, as well as with French and Italian. He attended law and other classes at Berlin University in summer 1859.

In 1860 he became acquainted with Sir David Brewster, having met him at his daughter-in-law Mrs Macpherson's house in Lasswade, and an intimacy and friendship sprung up between them, remarkable in several respects, and particularly in this, that young Deas was then barely one-and-twenty, while Sir David was in his eightieth year. The friendship thus formed subsisted during their joint lives, and was, I believe, a source of great pleasure and satisfaction to both, and certainly of great benefit to the younger of the two, though I venture to think that the benefit was mutual, as no one, and not certainly a man of Sir David's years and peculiar character, could fail to derive advantage from the simple and sincere affection of a youth so amiable and intelligent as Mr Deas. Sir David said of him from the first, that he had a more thorough and a more comprehensive hold of scientific principles than any man of his acquaintance not professionally scientific, and that he had so rare a combination of the faculties necessary for scientific research, that he (Sir David) deeply regretted "he was crippled by a profession so jealous as the law." Of the intimacy that thus arose very pleasing traces are to be found in the interesting volume of Sir David's Home life, by his daughter. In 1866 Sir David was seized when at Belleville with an unseasonable attack of hooping cough, and his illness was so severe as to excite the greatest alarm in Lady Brewster and his friends, although his mind remained bright, clear, and active. "A favourite young scientific friend," Mrs Gordon states, "Mr Francis Deas, was staying in the house at the time, and after hours of fatigue and suffering it was positive enjoyment to the invalid to make the little preparations for his visit, which was quite the event of the day. Believing himself a fast dying man, he left many instructions with Mr Deas as to the arrangement of his scientific instruments, &c., and two years afterwards, when the call really came, it was to this gentleman that he confided the finishing and reading of a paper for the Royal Society, which weakness prevented him from completing. It was on the Motion, Equilibrium, and Forms of Liquid Films."

Mrs Gordon gives us at the same time an interesting letter, written by Mr Deas to Mrs Macpherson after Sir David's death, sending his reminiscences of the three weeks spent by them together at Belleville on the occasion above referred to, and in a letter

written by Sir David on his death-bed, he refers to Mr Deas as the friend to whom he had entrusted the final preparation of the paper on Films already mentioned.

I may add, that there was found in Mr Deas's repositories, after his death, a letter to him from Mrs Macpherson, Sir David's daughter in-law, giving an account of his last moments, and referring to the scientific subject in question, on which, I believe, Mr Deas read a paper in this Society as requested. That letter will be found in an appendix to the notice I am now reading.

Mr Deas was admitted a member of the Royal Society in 1867.

He had previously passed advocate in May 1862. At a later period, he was the first to receive the new degree of LL.B. (instituted in 1862.) Upon that occasion he was presented for graduation by Professor Lorimer, with a well-merited tribute to his diligence and proficiency in law. He had thoroughly studied his profession, and continued to do so, extending his attention at the same time to various kindred branches of study, such as medical jurisprudence and anatomy.

He began now to contemplate the publication of some legal work that should be useful to him, and prior to 1870 was engaged in preparing a second edition of Mr Fraser's work on "Master and Servant," which appeared in January 1872. His laborious application however to that task, carried on in conjunction with the practice which he was obtaining at the bar, seems to have injuriously affected his health, and to have made the first encroachment that appeared upon his constitution, and in the summer of 1870 premonitory symptoms were observed of that tenderness of chest which ultimately proved fatal. By advice of his medical attendants he went abroad, in order to make what is called the Nile journey. He had twice before been abroad, and was thus not an inexperienced traveller. He much enjoyed the voyage up the Nile to the Second Cataract, and took an interest in all that he saw, visiting all the objects of celebrity within his reach. The atmospheric varieties of the country, and in particular the pure and inspiriting air of the Desert, seem to have done him good, as well as to have afforded him pleasure. His journal consisting of memoranda during this voyage, of which I have seen a copy, is very interesting, particularly to those who knew him, and shows how his

scientific tastes and feelings of curiosity were elicited at every step. It seems to be uncertain whether his health truly profited by this experiment. He appears to have doubted it himself; yet on arriving in London in June 1871, he wrote that there had been a marked change for the better ever since he recrossed the Alps, and that he was now so well that he wished to resume business. He returned to Edinburgh accordingly, and did resume business, but without attending the Parliament House. Upon putting out of hand his book on "Master and Servant," Mr Deas had commenced another work of a still more arduous kind, on the "Law of Railways," and to this he now applied himself as a professional task.

In February 1872 he again, by advice, went abroad, spending his time partly at Florence, but chiefly at Rome, still attending to all objects of interest, but at the same time continuing even there the progress of his book on Railways. He returned home in June of that year, perseveringly completed his book, and published it in January 1873. He very fittingly dedicated the work to his father, Lord Deas, "alike as a token of filial regard, and as a tribute to his acknowledged eminence as a lawyer." The book was received with great approbation, it evinces a wonderful degree of industry and energy, and cannot fail to be eminently useful to the profession, as many competent judges have gladly acknowledged.

In the narrative given above I have not said much of Mr Deas's scientific tendencies; but these, from the first, were very strong and decided. I have mentioned the opinion of Sir David Brewster as to the combination of qualities, which seemed peculiarly to fit him for scientific research, and his application to scientific subjects was constantly kept up. His reading was extensive in all the best books on science, and he contributed papers which were considered valuable to the best scientific periodicals of the day. He devoted a good deal of time to the study of optics, and had considerable practice in the use of the telescope; but was still more interested in microscopic investigations, in connection with which he amassed an extensive collection of objects for that instrument, nearly all prepared by himself, and accumulated during many years, wherever he travelled or happened to be.

It is to me a pleasant thing to record, and it must have been to his friends a great consolation to know, that in the midst of these

scientific investigations, which were fearlessly and searchingly conducted, he never lost sight of those great principles that connect the works of the Deity with His personal existence and moral perfections. Many entries in his private memoranda show his fidelity to these feelings, and prove that he shared with his friend Brewster the reverence for a Supreme Power which that distinguished man always evinced in the prosecution of his varied inquiries. Mr Deas's reading on sacred subjects seems to have been much in the Book of Psalms, a book which has proved a treasure and a favourite study with all the devout admirers of nature; and he often expresses in his memoranda how much the admiration felt by the authors of that book for the works of the Creator would have been exalted and enhanced, instead of being deadened or destroyed, by the new wonders revealed through the aid of scientific instruments.

It was not only to professional and scientific subjects that he directed his attention. He had, I think, a genius for music, and performed on the pianoforte with perfect taste and with a degree of skill that was scarcely to be expected from an amateur who had so many other avocations and pursuits of a more urgent and engrossing nature. He was also fond of sculpture and painting, and his friend, Sir Noel Paton, seemed to have pleasure in sending him his paintings before they were despatched to London, at a time when Mr Deas was, from illness, unable to leave the house.

After what I have said, I think I may confidently claim your sympathy with me in this tribute to the memory of a young man for whom, when he was in life, I felt a strong esteem and regard, in whose sad fate I saw a great private and public loss, and whose memory, I think, is entitled to our affectionate remembrance. Looking to his natural talents and tastes, to the assiduous cultivation that he bestowed upon them, to the variety of subjects to which his studies extended, and to the high and sound principles with which his mind was imbued, I venture to say, that I know of *no* young man who, if he had lived and had preserved a sufficient measure of health, was more likely to extend the range and maintain the dignity of science, as well as of mental culture generally, while at the same time I cannot help adding, and there is a satisfaction even in this feeling, that I know of no one who, from the innocence of his character and from the purity of

his feelings, as well as from the religious emotions which he carried into scientific investigations, was better prepared to be early removed from this temporary scene, seeing that such was the lot appointed for him. The loss of such a youth, who was doing so well, and promising so much more to be still done, must be a great affliction to all who knew him, and a very grievous one to those most nearly connected with him; but of such characters it is the privilege of survivors to speak, as good men have often done, that the memory of the departed is a treasure that cannot be outweighed even by present blessings.

APPENDIX.

Letter referred to on page 463, from Mrs Brewster Macpherson, found in Mr Deas's repositories, dated Allerley, Monday, February 10th, 1868.

"You will, I know, be intensely anxious to hear of dear, dear papa. Sir James Simpson says he cannot live over the night. We got a train straight on to Melrose on Saturday, so I gave my note to a porter to post for me. I hope you got it. We found Sir David much stronger and better than I had expected, so much so that I could not believe he was dying. He slept all that night, and up to twelve on Sunday. I could not believe he was dying, then he sank very rapidly. His perfect trust in the love of God, and in the finished righteousness of the Saviour, is wonderful. He has no wish to live—no fear of death—absolutely none. His faith is pure and childlike. His mind is perfectly clear. He expressed a wish twice to me that you should finish a paper which he had begun on Soap-bubbles, and read it for him at the Royal Society. He expressed the same wish to Sir James Simpson last night, and he has left a paper for you with instructions about it. Lady Brewster wrote at his request on Friday. He has spoken of you repeatedly to me with such kindness. Oh! Frank, it was awfully solemn all yesterday, and how much more so to-day—one of the great lights of the world going out."

4. Biographical Notice of Adam Black. By the Rev. Dr Lindsay Alexander.

Adam Black was a native of Edinburgh, where he was born on the 20th of February 1784. He received his education at the High School of the city, and afterwards attended for two sessions the classes at the University. Having selected bookselling as his profession, he became apprentice to Mr Fairbairn, an Edinburgh bookseller, and at the close of his apprenticeship spent two years in the house of Lackington, Allen, & Co., London. In 1815 he commenced business for himself in Edinburgh as a bookseller; and entered upon that career of wise and vigorous enterprise which he pursued to the end of his life, and in which, both as a man of business and as a public man, he earned for himself a wide-spread reputation. When the first Town Council under the New Municipal Act was elected, he was returned as one of the councillors; shortly after he became treasurer of the city funds, and laid the foundation of that scheme by means of which the pecuniary affairs of the city were at length brought into order, and the city relieved of the pressure of debt; and in 1843 he was raised to the office of Lord Provost, an office which he held by re-election for six years. On his retirement from this office he was offered a knighthood by the Government, but this he declined, alleging that as he was still in business as a retail bookseller and stationer, it would be incongruous for him to be standing behind his counter to be addressed there as "Sir Adam" by some boy sent up from the market "for a hard pen and a pennyworth of ink." In 1856 he was returned to Parliament as one of the members for the city, and to this dignified post he was repeatedly re-elected, and represented the city for nearly ten years. On his retirement from Parliament he still continued to take an interest in public affairs, as well as in the conduct of his business. For some years he had been withdrawing from bookselling and confining his energies and resources to publishing. By a happy union of boldness with prudence he raised his house to a foremost place among the great publishing firms of the country. Two large editions of "Encyclopædia Britannica," each of which was nearly all written anew, and numerous editions of the "Waverley Novels," and other writings of Sir Walter Scott, in various sizes

and at prices that brought these matchless productions within the reach of all classes of the community, attest the vigour and skill with which he carried on his enterprise as a publisher. To him also is due the honour of being the first to summon the learning of the churches to the preparation of a "Cyclopædia of Biblical Literature," such as should present in a condensed form the results of the most advanced investigation into the history, literature, and archæology of the sacred writings. These are but a very few of the works he published, but they are the most important; of the rest it may be said generally, that they all possess some quality of excellence such as makes them valuable contributions to the literary or scientific products of the day.

Mr Black died on the 24th of January 1874, having nearly completed his 90th year. Not only for the services he rendered in various ways to the city, not only for his abilities and his success in business, not only for his enterprise and wisdom as a publisher, but still more for his moral qualities, his perfect integrity, his transparent honesty, his steadfast consistency, his unaffected piety, and his unswerving loyalty to truth and equity, will his name be handed down to posterity by the people of this city as that of one of the noblest and worthiest of her citizens.

5. Biographical Notice of Sheriff Cleghorn. By David Maclagan, Esq., C.A.

Thomas Cleghorn was born in Edinburgh 3d March 1818, and died there 13th June 1874. His father, Alexander Cleghorn, Collector of Customs, was an esteemed citizen of Edinburgh; his uncle, David Cleghorn, was long Crown Agent; and a second uncle, the Rev. Thomas Cleghorn, was parish minister of Smailholm, of which his great-grandfather, Dr Duncan, had also been pastor.

Mr Cleghorn was educated at the Edinburgh Academy and at the University, in both of which he was distinguished by earnest application and by high character. His favourite study was that of natural philosophy, and in the distinguished occupant of that chair, James David Forbes, he found a life-long friend and correspondent. Mr Cleghorn wrote a cordial and discriminating notice

of Forbes after his death, in one of the magazines of the day. Like most of the foremost students of the University he was a member of the Speculative Society, and in later years, along with his friend Mr Robert Balfour, now deceased, wrote its history, a work of great research and interest.

Mr Cleghorn was called to the Scottish bar in 1839, and held successively the offices of Advocate-Depute, Registrar of Friendly Societies, and Sheriff of Argyle, which latest appointment he continued to hold until his death. He was unanimously elected in 1871 Legal Adviser of the Free Church of Scotland, of which he was an attached member and office-bearer. Mr Cleghorn's connection by marriage with the family of the late Lord Cockburn introduced him to a highly cultivated literary circle, in which he was well fitted, by his classical and scientific knowledge and wide range of literary study, to occupy a place. For very many years Mr Cleghorn devoted much time to the advancement of educational, benevolent, and religious objects, to all of which he was a most liberal contributor. The welfare of schools and colleges generally was always a source of interest to him, while the Edinburgh Academy, of which he was for many years a Director, and the University of his native city, were specially dear to him.

Wellington School, an institution for the reformation of young criminals, was founded by him, and to its support he largely contributed both means and personal labour.

Mr Cleghorn has left a name greatly esteemed, and will be remembered as a man of much culture and many acquirements, as well as a citizen of proved worth and of large hearted public spirit.

6. Biographical Notice of Henry Stephens. By Professor Maclagan.

Mr Henry Stephens was in the Royal Society essentially the representative of the important science of agriculture, and has left behind him a reputation as an agriculturist not confined to Britain, for his works on agriculture have been translated into every European tongue, and are thoroughly appreciated abroad. He was born in July 1796, in Forfarshire, where he inherited the

estate of Balmadies. He seems, from his earliest youth, to have had an enthusiastic love for agriculture, and to have from the first regarded it not as a business to be conducted by empirical or routine rules, but as an art to be practised under the guidance of scientific principles. He intended that he should be a practical farmer, but he resolved that to fit himself for this he should make himself a well-educated gentleman. His motto seems all along to have been "thorough," and his guiding rules diligence and method. Nothing can illustrate this better than a manuscript volume which he left behind him, bearing on its title page, "A Course of Education, comprising Mathematics, Natural Philosophy, Natural History, Chemistry, and Agriculture. Dundee, 1815." The volume, which looks almost as if he intended it to be printed as a text-book for young agriculturists, was begun by him when he was 19. It is not original work, but consists of notes taken by him during his attendance on courses of instruction, of which he gives the following account in a formal preface to his manuscript volume:—

"The notes on mathematics, natural philosophy, and the outlines of chemistry, were taken at the lectures of Mr Duncan in the Dundee Academy, from 1st October 1809 to 1st August 1810, and from 1st October 1810 to 1st August 1811, which completed the session at the academy.

"The notes on chemistry were taken when attending the lectures of Dr Charles Hope in the University of Edinburgh, from 6th November 1812 to the 26th April 1813. Those on natural history, when attending the class of Mr Robert Jameson, in the same place and during the same period. In the same place the lectures on agriculture by Dr Andrew Coventry, commenced 5th January 1813 to 28th April of the same year; but during that period [I] attended his class twice a day, at 8 o'clock in the morning and at 3 o'clock in the afternoon."

This preface is a true index of the character of the man, even as he was known in his old age—complete methodicity, unsparing energy, and perfect precision in everything.

Stephens had, by theoretical preparation, made ready for cultivating his own estate, but he felt the necessity for practical study also, and therefore he placed himself, with a view to learning his work practically, with one of the largest and most skilful agricul-

turists in the county of Berwick, which had then the repute of being the best farmed district of Scotland. On this farm—Whitsome Hill—he remained for three years, engaging, as he himself records, "in every sphere of work which the ploughman, the shepherd, and the field-worker must perform in the field, or the steward or cattleman at the steading;" even in the dairy and poultry-house part of his time was spent; and all this he undertook "not of necessity, but voluntarily, and with cheerfulness, in the determination of acquiring a thorough practical knowledge of his profession."

Thus armed, he was prepared to encounter the work of cultivating a part of his own estate, and he soon saw that to do this satisfactorily a considerable expenditure of money was called for; and this was done, to the effect of raising the value of the farm which he personally worked, from L.150 to L.400 a year. But evil days were in store for him. By the failure of an Indian house in which his money was invested, and just at the time when he had spent much on improving his property, he was straitened in his means, and he had to bethink himself of other ways of carrying out his life's object of being an agriculturist. It was at this time, when he was under the cloud of misfortune, that an accident occurred which laid the foundation for his reputation as an agricultural author. He was travelling in the coach from Dundee to Edinburgh when he encountered, as travelling companion, the eminent founder of the great publishing-house of William Blackwood and Sons. The sagacious William Blackwood was too acute not to perceive that in his young travelling companion he had found a man thoroughly versed in the science of agriculture. He shortly after called Stephens to his aid in conducting the Journal of Agriculture, and thereby was commenced a literary connection with the Blackwoods, which has extended even to a third generation. It was through them that he gave to the agricultural world his "Book of the Farm," the first edition of which was published in 1842, and a second edition in 1871—the manuscript of which,—almost a complete re-writing of the original edition,—was worked up with the same precision, attention to detail, and neatness of penmanship, which characterised the "Course of Education" of 1815. His other works were—in conjunction with Mr G. H.

Slight, "The Book of Farm Implements and Machines;" in conjunction with Mr R. Scott Brown, the "Book of Farm Buildings;" in conjunction with Dr Seller, "Physiology at the Farm;" the "Manual of Practical Draining;" the "Yester Deep Land Culture;" and the "Catechism of Practical Agriculture."

He was an original and active member of the Meteorological Society of Scotland, and, although not writing much on the subject, he was in constant communication with the Secretary of the Society, especially in giving advice and assistance in all questions of meteorological science which had a special bearing on agriculture.

Mr Stephens, for many years previous to his death, was in the habit of repairing annually for the recruitment of his health to Homburg, and, in the course of his various visits to Germany, visited all the more celebrated vine growing districts on the Rhine. He carried his agricultural spirit with him in all these trips, noting all the processes of vine cultivation, even to its minutest details, and bringing back with him an ever increasing appreciation of all the best vintages of the Rhine, of which he always possessed a modest but select store, with which he delighted to refresh any friend visiting him at Redbraes, whom he thought capable of fully estimating his favourite wines. He had, however, even better entertainment for his visitors in his conversation, which was to the last full of good nature, with a large spice of "pawky" humour, sometimes in his later years a little prolix, but always yielding something in the way of anecdote or scientific—especially agricultural—observation worth listening to. For many years he had been made aware that he had a certain amount of organic change of structure in the aortic orifice of the heart; but this made no progress, and, so far as it was concerned, he might have prolonged his days. His death, however, was ultimately due to accident. It is remarkable that he was three times the subject of poisoning. He was one of the first of several instances which have occurred of poisoning by the flesh of American partridges, and his case was graphically narrated by his then medical attendant and friend, the late Dr Burt. He, on another occasion, suffered a good deal by the inhalation of coal gas which had escaped in his bedroom during the night, but from this he soon got well. It was, however, a repetition of this accident which ultimately led to his death. On

the night or early morning of 21st June 1874 he had, as he thought, extinguished the gas in his small bachelor bedroom, but unfortunately had left the stop-cock open, and it was his not making any movement in the morning that attracted the notice of the servants; one of them entering his room found him insensible, in an atmosphere strongly charged with gas, and, seeing at once what had happened, sagaciously opened the window, and got him to swallow some stimulant. His medical attendants succeeded in rousing him from his comatose state, and he seemed in the fair way of recovery, but a low congestive inflammation of the lungs supervened, and proved fatal on the 4th of July.

7. Biographical Notice of Christopher Hansteen. By Alexander Buchan, Esq.

Christopher Hansteen was born at Christiania on the 26th of September 1784. In 1802 he entered the University of Copenhagen as a student of law, which, however, he soon abandoned for what was to him the more congenial study of mathematics. He became mathematical tutor in the Gymnasium of Fredericksburg, in the Island of Zealand, in 1806, and about the same time he gained the prize which had been offered by the Royal Society of Sciences of Copenhagen for the best essay on terrestrial magnetism. Shortly thereafter, viz., in 1814, he was appointed to the chair of astronomy in the University of Christiania, which had recently been founded by Frederick VI. of Norway.

He continued to prosecute his researches into terrestrial magnetism with ardour and success, the results of which appeared in his great work, entitled "Untersuchungen über den Magnetismus der Erde," which was published in 1819 by the liberality of the King of Norway. The work was illustrated with an atlas of maps, and besides containing the fullest and best collection of observations on terrestrial magnetism which had then appeared, it was remarkable for great breadth of treatment and sound philosophical generalisations.

In continuing the prosecution of his physical researches, he made a journey into Siberia, accompanied by Ermann and Due, the expenses of the expedition being defrayed by the Norwegian

Government. One of the most important results of this expedition was the establishment, on Humboldt's recommendation, of the ten magnetical and meteorological observatories by the Emperor of Russia, at which hourly observations were recorded for many years, and annually published *in extenso* by the Russian Government, the whole forming the completest record of these phenomena we yet possess.

Shortly after his return from Siberia the Norwegian Government voted the funds for building an astronomical and meteorological observatory at Christiania, which was erected under Hansteen's direction. He also superintended the trigonometrical and topographical survey of Norway, which was begun in 1837.

The completion of his fifty years' public services was commemorated in 1856, shortly after which he ceased to lecture, and in 1861 retired altogether from public duty. He died on the 11th April 1873, at the advanced age of 88.

8. Biographical Notice of Jacques-Adolphe-Lambert Quetelet. By Alexander Buchan. Esq.

Jacques-Adolphe-Lambert Quetelet.—On 17th February 1874 Quetelet died at Brussels, in the seventy-eighth year of his age, having been born at Ghent on 22d February 1796. At the age of 18 he was appointed Professor of Mathematics in the College at Ghent; and in July 1819, the degree of Doctor of Science was conferred on him by the University of the same town, which had just been founded by King William. His dissertation on the occasion was so well received that he was shortly thereafter appointed to the Chair of Mathematics in the Royal Athenæum of Brussels. In February following he was elected a member of the Academy of Sciences and Belles-Lettres.

The earliest of Quetelet's published memoirs, which began to be issued in 1820, were on geometrical subjects. He soon, however, directed his attention more exclusively to physics and astronomy, and lectured publicly on these subjects with great success.

In 1823 he was sent to Paris to report on the observatory of that city, for the guidance of the Belgian Government in founding a similar observatory at Brussels. After some delay the observatory

was founded, with Quetelet as director; and in 1833 were begun the valuable series of astronomical, meteorological, and other physical observations for which this observatory is so favourably known. Of the work done by this observatory, special mention may be made of the catalogue, begun in 1857, of stars which seem to have appreciable motion; and the systematic observation and publication, from 1836, of the occurrence of meteors and shooting-stars,—records which proved to be of so great value thirty years later when the true character of these bodies was satisfactorily established. The meteorological observations have been particularly full and valuable, and they have been exhaustively discussed by Quetelet in his "La météorologie de la Belgique comparée à celle du globe," published in 1867,—a treatise which must yet be regarded as the fullest and best account of the meteorology of any single locality on the globe. Stations at Liège, Ghent, and other places in Belgium, were also established by him in 1835.

He was elected Perpetual Secretary of the Academy of Sciences and Belles-Lettres in 1834, and to his influence was chiefly due the section on the Fine Arts which was added to the Academy in 1845. To this section Quetelet made extensive and original contributions, particularly in his researches regarding the proportions of the human body, the results of which are published in his "Athropométrie." In matters referring to the higher education of the people, the census, and several other national questions, the Belgian Government availed itself repeatedly of his great knowledge and experience.

He was made President of the Central Commission of Statistics at its establishment in 1841, and continued President till his death. His first paper on Statistics was published in 1826; in 1835 appeared his "Physique Sociale," and ten years later his "Lettres sur la théorie des Probabilités appliquées aux sciences, morales, et politiques." He originated the idea of convening an International Congress of Statistics, and the first Congress was held at Brussels in 1853.

The many-sidedness and fertility of Quetelet's genius may be seen from the list of his scientific memoirs, enumerated in the Royal Society's Catalogue, amounting at the close of 1863 to 220. It is in the field of statistics that Quetelet appears as a great dis-

coverer, and his success in this department is to be attributed to the clearness with which he saw that statistics occupy the same place in the development of the social and political sciences that observational data do in the development of such sciences as astronomy and meteorology; to the patient industry with which, through long years, he gathered together his facts; and to the mathematical skill he brought to bear on their discussion. He was truly, as stated by the Academy of Berlin in their congratulatory letter on the occasion of the centenary of the Belgium Academy, "the founder of a new science, which proceeds from the firm basis of observation and calculation, to discover and unfold those immutable laws which govern the phenomena, apparently the most accidental, of the life of man, down even to his most trivial actions."

9. Biographical Notice of George Berry. By George Barclay, Esq.

Mr George Berry was born in Edinburgh (where his father, of a Quaker family in Somersetshire, had settled as a merchant), on the 12th of January 1795. Bred to business himself, partly at home and partly in France, Mr Berry succeeded his father in Edinburgh, but about 1834 removed to Leith, whence, after a successful mercantile career of twenty years, he retired, and died at Portobello on the 1st of May last.

While in Leith Mr Berry took an active part in public affairs; he was one of the founders of the Chamber of Commerce, and having early become an enthusiastic "Free trader," he continued, during the years of struggle which preceded the national adoption of that policy, perhaps the most prominent representative of free trade doctrines in Leith.

But though greatly occupied with business, Mr Berry was through all his life also somewhat of a student. A great reader, and gifted with a retentive memory, he was well versed in English literature and in science. He had been a pretty good chemist of his own day, but specially a devoted and accomplished mineralogist and geologist of the school of Jameson. In pursuit of these studies he spent for years as a young man his spare hours at home, and his holidays in

wanderings after "specimens," in the then little travelled Western Highlands, of which he had many curious anecdotes to tell; following his master, he became a keen "Wernerian" in those days of hot geological controversy. He was admitted to the membership of the Royal Society in 1861.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VIII. 1874–75. No. 91.

NINETY-SECOND SESSION.

Monday, 1st February 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Complete Theory of the Stone Arch. By Edward Sang, Esq.

In the investigations usually given of the equilibrium of the stone bridge, it is assumed that the strains follow the same law as in the suspension bridge, the one being a case of stable, the other a case of instable, equilibrium; and, resulting from this hypothesis, formulæ are given whereby to determine the *extrados* suitable to a prescribed *intrados*.

The load of the chain bridge is suspended by rods, and therefore acts only in the direction of gravity; it cannot influence the horizontal strain which must be transmitted unaltered from link to link. But the weight is imposed on the stone arch in a different manner. The stone which rests on the voussoir is not jointed as the suspending rod is, and may transmit an oblique as readily as a downward strain: hence the fundamental conditions of the two structures are essentially different, and require different modes of treatment.

The mason trusts greatly to the cohesion of the cement, which easily makes up for small inaccuracies of arrangement; but the maximum stability of a stone structure is attained by so adjusting its parts that each would be in equilibrium even although the sur-

faces had no cohesion and no friction. For this we require that the thrusts to which each stone is subjected should be in directions normal to its several surfaces, and should balance each other.

Now each arch-stone is subjected to three pressures,—one on each of its sides in directions tangent to the curve of the arch, and a third, the pressure of the superincumbent mass upon the inner end of the stone.

To put the structure in accordance with the usual supposition, we must cause the inner ends of these stones to be dressed with horizontal surfaces, in order that the pressures exerted thereon be downwards. This being done, the usual investigations would hold good, and the intrados for a rectilinear extrados would be a modification of the catenary. But the inner ends of the arch-stones are never dressed in this way; they are rough-hewn, and made parallel to the curve of the arch, and thus the deductions from the usual hypothesis are quite inapplicable.

If we suppose the inner ends to be made parallel to the arch and to be frictionless, the load resting upon them would tend to slide down the slope, and this tendency must be counteracted by a horizontal resistance from the adjoining masonry; this, combined with the gravitation of the load, produces a resultant normal to the arch. In this way the compression of the arch stones is transmitted unchanged along the whole curve, instead of being, as in the former case, augmented in proportion to the secant of the inclination; and at the same time the horizontal thrust, instead of being conveyed unchanged to the ultimate abutment course and there resisted, is distributed through the whole depth of the mason work. On investigating that form of the intrados which, on this supposition, must suit a horizontal roadway, we obtain a differential equation which can only be integrated in somewhat complex series. This curve lies inside of the circle which osculates the arch at the vertex, while the catenarian curve, resulting from the former hypothesis, lies entirely without that circle. Between these two curves, therefore, we may have a variety of intermediates, each of which may be brought strictly into accordance with the laws of equilibrium by giving to the inner ends of the arch stones an appropriate degree of inclination.

In this way we are at liberty to assume, within reasonable limits,

the forms of both intrados and extrados, and at the same time are able to satisfy punctiliously the conditions of equilibrium by properly adjusting the slope of the inner surface of the arch stone. The computation needed for this adjustment is simple and obvious.

The builder, however, would scarcely think it worth his while to cut the stones truly to the shape so found; he would often prefer the usual rough-hewn surface and the great cohesion which that roughness gives; and will probably rest contented with a test for safety, which test may be found in the very simple law, that the difference between the logarithms of the tangents of the inclinations at the two proximate points of the arch should always differ by more than the logarithms of the loads imposed between those points and the vertex of the structure.

2. On the Application of Angström's Method to the Conductivity of Wood. By C. G. Knott and A. Macfarlane. Communicated by Professor Tait.

This was an account of a series of experiments made in the Natural Philosophy Laboratory of the University, to test the applicability of Angström's method of periodic variations of temperature to the determination of low conductivity. The wood was cut into discs of a standard thickness, and these were very tightly secured together, after the interposition of copper-iron thermo-electric junctions (of very fine wire). One series of discs was cut parallel, the other perpendicular, to the fibre. The results were obtained very easily, and accorded satisfactorily with those obtained by more laborious methods.

3. Notice of Striated Rock Surfaces on North Berwick Law. By David Stevenson, V.P.R.S.E., Civil Engineer.

The well marked "crag and tail" formation of North Berwick Law has long been appealed to by geologists as a striking example of the effects of those mysterious glacial currents, which at some time have wrought such changes on the surface of the globe. The Law presents, as is well known, a comparatively bold face, or crag

to the west, against which the glacial current is supposed to have impinged, while, against its eastern face, there is a gently sloping mass of gravel, clay, and stones supposed to have been thrown up by this current under "lee" of the Law, and now forming what is called the "tail." It had often occurred to me as remarkable, that so great a mass of debris should have been left by the passing current, whatever that may have been, on the eastern extremity of the hill, while it had apparently left no impression on the north and south sides, along which it must have passed. These north and south sides, in their present condition, and to a casual observer, have the ordinary appearance of rough, angular weather-beaten rocky faces, without a trace of glacial action. However, when making an engineering examination of the country around North Berwick in September last, in search of an available water-supply for the town, I found certain very distinct traces of glacial action on the northern side of the Law, which, in connection with the "crag and tail" feature to which I have referred, must, I think, be interesting to the geologist, while they may possess additional interest from their being on a steeply inclined open hill face, and not in a ravine, or on nearly horizontal or slightly inclined strata.

They extend vertically over a space of about 30 feet, indicating the action of a moving mass of at least that depth. They can be traced horizontally over a space of about 200 feet, and they range from 160 to 190 feet above the sea-level. They present the usual two-fold glacial aspect of smoothly-ground undulating surfaces, indented by occasional deep striæ or scorings. These two kinds of marking may have been made at the same, or at different periods, but the same abrading agent could not have produced both of them. The grinding or dressing, as it has been termed, of the surface is very distinctly marked, and must have been done by the passage of some dense, but yielding body, which could be moulded to the different irregularities, both vertical and horizontal, in the surface of the hill, which must obviously have been subjected to the grinding action for a considerable period before the observed effect could have been produced. The striæ again, must have been made by the passage of sharp-pointed bodies, harder than the felspar porphyry of the Law, and carried in a mass of material of sufficient density firmly to retain the sharp, rocky protuber-

ances embedded in it, and to press them against the hillside with enormous force, so as to groove the rock face in passing. As viewed from a little distance, the scorings appear to be nearly paralled and horizontal. But on examining such as can be reached, I found, on using the clinometer, that this is by no means the case. On one patch of rock I found two striæ within 18 inches of each other, the upper of which had a dip of 4° and the lower a dip of 20°, and both markings were dipping towards the west, being the direction from whence the movement came, as indicated by the "tail" on the eastern side of the Law. But the rise in the direction of motion indicated by these two striæ may have been caused by local pressure, due to the obstructions offered to the passage of the mass by the Law. The effect of this would be to elevate the mass; and this I think points to ice carrying imbedded stones as probably the agent which has so distinctly chronicled its passage over or round the hill, while the rise on these lines indicate that the moving mass must have been under enormous pressure; and this again is perhaps sufficient to account for the cutting of the deep grooves left in the rocky face. In short, the appearances I have noticed seem to be such as might naturally result from such glacial action as Forbes has recorded, when he says, "when the ice of the glacier abuts against the foot of Mont Chetif it is violently forced forward, as if it would make its way up the face of the hill." *

The markings I have described have, till a comparatively recent period, been covered by debris, which has fallen from the upper portion of the Law, and formed a glacis at its base. The removal of a portion of this debris, extending to about 200 feet, as a quarry for road metal, has disclosed the original surface of the rock, and revealed the features I have described. A similar mass of debris extends along the whole northern and southern faces of the hill, and if it were removed, I have no doubt similar markings would be found to extend along both sides. I believe some traces of glacial action have been found at a high level on the western face of the Law; but I carefully examined the north and south faces of the hill, and could not, in their present buried up state, find

* Travels in the Alps, page 205.

traces of markings, except at the place I have described, from which the debris has been removed. Neither could I detect markings on the rock faces immediately above the striated surfaces, but these upper faces having been exposed to the weather, and never covered by debris, might, though at one time scored, gradually lose the markings, while those in the lower portion of the hill remained protected by the debris.

The existence of these markings seems to supply another link in connecting the "crag and tail" formation with glacial action, at least in the case of North Berwick Law. From the appearances which the removal of this debris have disclosed, we are warranted in concluding, that after the passage of the ice-sheet or glacial current, the rocky face of the Law, perhaps to its whole height, depending on the depth of the abrading agent, was similarly rough-polished and scored, that these markings on places exposed to atmospheric action have been gradually destroyed, while similar markings on the base have been preserved by the covering of debris, and may now be seen almost in their original state, if not of freshness, as least distinctness of marking.

If this view be correct, it is likely that by removing similar deposits from the base of Stirling Castle, Craigforth, and other similar rocks, interesting traces of glacial action in connection with the "tails," which exist at these places would be disclosed.

4. Laboratory Notes. By Professor Tait.

a. Photographic Records of the Sparks from a Holtz Machine.

To determine the cause of the ordinary zig-zag form of electric sparks, the author requested Mr Matheson, one of his laboratory students well skilled in photographic processes, to take instantaneous photographs of the sparks of the Holtz Machine, by means of a quartz lens, in hot and cold air alternately. Several of these photographs were exhibited, and showed much greater smoothness of the track of the spark in heated than in cold air. The zig-zag appearance seems to depend upon the presence of combustible organic particles in ordinary air, but the experiments are still in process.

b. Determination of the Surface-Tension of Liquids by the Ripples produced by a Tuning-Fork.

A slight modification of a formula given by Sir W. Thomson (*Phil. Mag.* ii. 1871), shows that the period (t) of oscillation of a particle in a deep mass of liquid agitated by simple waves or ripples is

$$t = \frac{\lambda}{\sqrt{\frac{g\lambda}{2\pi} + \frac{2\pi}{\lambda}\frac{T}{\rho}}}$$

where λ is the wave-length, T the surface-tension, and ρ the density of the liquid. By producing, with the aid of a massive tuning-fork, steady ripples in various liquids all subjected to the same conditions, and measuring micrometrically the length of these ripples, the quantity T is determined with considerable accuracy from the above formula.

c. Capillary Phenomena at the Surface of Separation of two Liquids.

The only difficulty in this investigation is the selection of two liquids, neither of which will line the interior of the capillary tube so as to disturb the behaviour of the other. This was effected in various ways, most simply by employing water and sulphuric ether; for when these liquids are shaken together and allowed to come to rest, the result is the production of a very sharply defined bounding surface between a weak solution of water in ether (above) and a weak solution of ether in water (below). The observations and measurements were made with contiguous portions of the same capillary tube,—one dipping into the upper, the other into the lower, layer.

The following Gentlemen were duly elected Fellows of the Society :—

Robert Clark, Esq.
The Hon. James Bain, Lord Provost of Glasgow.
Dr T. S. Clouston, F.R.C.P.E.
Thomas Fairley, Esq., Lecturer on Chemistry, Leeds.

Monday, 15th February 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Obituary Notice of Dr Robert Edward Grant, late Professor of Comparative Anatomy in University College London. By Dr W. Sharpey.

Dr ROBERT EDMOND GRANT was the seventh son of Alexander Grant, Esquire, Writer to the Signet. He was born in his father's house in Argyle Square, Edinburgh, on the 11th of November 1793. His mother's maiden name was Jane Edmond. It appears from a memorandum in Dr Grant's handwriting, that he was sent from home to be nursed, and saw little of either of his parents during his infancy and childhood. He had eight brothers and three sisters, all of whom died before him, and as none of them left any children, Dr Grant was the last survivor of his family.

When about ten years old he was placed at the High School of Edinburgh, where he continued for five years, under the tuition, successively of Mr Christison, afterwards Professor in the University, Dr Carson, and Dr Adam, the Rector, author of the well-known work on Roman Antiquities. In 1808 his father died, and in November of that year, Dr Grant became a student in the University of Edinburgh, attending the junior classes of Latin and Greek. In the following November he entered on his curriculum of medical study, and during its course attended the several classes in the Faculty of Medicine under the professors of that day. He also studied Natural History under Professor Jameson, and attended the lectures of some of the extra-academical teachers. After completing his course of medical study, he, in 1814, took his degree of Doctor of Medicine, and published his inaugural dissertation, under the title "De Circuitu Sanguinis."

In the meantime he had obtained (in May 1814) the diploma of the College of Surgeons. In November of the same year, he was elected one of the presidents of the Medical Society of Edin-

burgh, a place justly regarded as an honourable object of ambition among the young aspirants in the Medical School.

Rather more than a year after taking his degree, Dr Grant went to the Continent, where he spent upwards of four years. During this time he visited various places of interest in France, Italy, and Germany, and made a pedestrian tour in Hungary; but his principal stay was in Paris, Rome, Leipsic, Dresden, Vienna, and Munich, on account, no doubt, of the varied opportunities for scientific study and general culture afforded by these foreign seats of science, art, and learning. He returned to Edinburgh in the summer of 1820, and took up his residence in his native city. At a later time he became a Fellow of the Edinburgh College of Physicians, but he seems not to have engaged in medical practice; his career had taken another direction. He had early imbibed a taste for comparative anatomy and zoology, and now devoted himself assiduously to the prosecution of these branches of science, both by continued systematic study and by original research. The study of the invertebrate animals was peculiarly attractive, and at this time Dr Grant published various interesting anatomical and physiological observations on mollusks and zoophytes; and his name will always be associated with the advances of our knowledge concerning the structure and economy of sponges, to the investigation of which Dr Grant at this time enthusiastically applied himself. The pools left by the retiring tides on the shores of the Firth of Forth afforded favourable opportunities for observation, and he would spend hours patiently watching the phenomena exhibited by these humble organisms in their native element.

Dr Grant remained at Edinburgh till 1827, and in the meantime communicated the results of his various scientific inquiries to the Edinburgh Philosophical Journal and to the Memoirs of the Wernerian Society, of which he became an active member. He was also in 1824 elected a Fellow of the Royal Society of Edinburgh.

In June 1827 Dr Grant was elected Professor of Comparative Anatomy and Zoology in the newly founded University of London, afterwards University College. He was not altogether new to the work of teaching. He had some early, though brief, experience in Edinburgh in 1824, when Dr Barclay, who for some years had delivered lectures on Comparative Anatomy during the summer

session, entrusted him with the part of the course which related to the anatomy of invertebrate animals. He entered on his duties in London in 1828, and in October of that year delivered his Inaugural Lecture, which was published at the time, and went through two editions. In this office he continued up to the time of his death, during which long period of forty-six academical years he never omitted a single lecture. This was a point on which he justly prided himself. Up to the last session (1873-74) he continued to give five lectures a week, but, sensible of failing strength, he proposed to reduce the number to three in the next session, which he was not destined to see. The number of students who entered to his class fluctuated a good deal, but was never large, attendance not being compulsory in the medical curriculum prescribed by the licensing corporations. In one session (1836-37) the number was fifty-six, but usually it was between thirty and forty, and sometimes much less.

After he had thus laboured for more than twenty years, the Council of the College added to the small return he received for his services an anual stipend of one hundred pounds, which was continued during the rest of his incumbency. About the same time a number of his friends, in presenting him with a microscope, in testimony of their esteem, purchased for him a Government annuity of fifty pounds. Afterwards he succeeded to some property left by his brother Francis, an officer in the Madras army, who died in 1852, so that in his later years he found himself in easy circumstances.

His leading pupils were much attached to him, and he was sincerely esteemed and respected by all. His style of lecturing was clear and impressive, with a ready and copious flow of language. Without meaning to speak of his mode of treating his subjects, we may nevertheless remark, that on one great biological question—the origin of species—he was from the first an evolutionist, and on the promulgation of the Darwinian hypothesis of natural selection he became one of its warmest adherents.

Between 1838 and 1840, Dr Grant was frequently engaged to deliver lectures at the Literary and Scientific Institutions of various large provincial towns, where his services were in great request and high esteem. In 1833 he gave a gratuitous course of 40 lectures,

on the structure and classification of animals, to the members of the Zoological Society. In 1837 he was appointed Fullerian Professor of Physiology in the Royal Institution, which he held for the usual period of three years. At a later period he was appointed by the Trustees of the British Museum to the Swiney Lectureship on Geology, the tenure of which is limited to five years. In 1841 he delivered the annual oration before the British Medical Association. In 1836 he was elected a Fellow of the Royal Society of London. He was also a Fellow of the Linnean, Zoological, and Geological Societies.

Dr Grant's vacations were spent sometimes in Scotland, but chiefly abroad, in France, Germany, Belgium, and Holland. On more than one of these occasions he was accompanied by an intelligent and favourite Hindoo pupil, Dr Chuckerbutty, who afterwards became a Professor in the Government Medical College of Calcutta. Dr Grant seems to have had a special liking for Holland, which he visited and revisited several times, partly no doubt on account of its scientific institutions and zoological museums, but largely also for the sake of acquiring the Dutch language. In like manner he afterwards spent a vacation in Copenhagen, and worked hard at Danish. Indeed, it is to be noted that he had a great taste for the study of languages, both practical and philological, and spoke the principal European tongues fluently.

Dr Grant's lectures were reported in the early numbers of the "Lancet," and he afterwards published a treatise on Comparative Anatomy, which embodied the substance of them. The work came out in parts. but was not completed. He was also author of the article, "Animal Kingdom," in Todd's Cyclopædia of Anatomy. The titles and dates of his communications to periodical works are given in the Royal Society's Catalogue of Scientific Papers; they are thirty-five in number, and extend from 1825 to 1839.

Dr Grant was a devoted lover of music, and attendance at operas and concerts was one of his chief enjoyments in his latter years.

In August 1874 Dr Grant suffered from a dysenteric attack, for which at first he would have no medical advice, and although subsequently, by appropriate treatment, the virulence of the disease was subdued, his strength was exhausted, and he died on the 23d of that month, at his house close by Euston Square. He was

buried in Highgate Cemetery, attended to the grave by a few old friends and attached pupils, among whom was his friend and former companion in travel, Dr Chuckerbutty, who was then in England, and two months later was destined to follow his venerated master.

Dr Grant was never married; he knew of no surviving relative. Three of his brothers, whose deaths he had recorded, were military officers. Of these, James, a lieutenant in the German Legion, fell at the siege of Badajoz in 1811; Alexander, captain in the Madras Engineers, died in the Burmese War in 1825; and Francis, captain in the Madras army, as already mentioned, died at Edinburgh in 1852.

By his will Dr Grant bequeathed the whole of his property, including his collections and library, to University College, in the service of which he had spent the greater part of his life, and to the principles of which he was sincerely attached.

2. An Illustration of the relative Rates of Diffusion of Salts in Solution. By Professor Crum Brown.

3. On the Oscillation of a System of Bodies with rotating Portions. By Sir William Thomson.

4. Laboratory Notes. By Prof. Tait.

a. On the Application of Sir W. Thomson's Dead-Beat Arrangement to Chemical Balances.

A considerable amount of time is lost in making an accurate weighing on account of the slowness of oscillation of the balance when the loads are nearly equal; and this loss of time is nearly proportional to the delicacy or sensitiveness of the balance. Hence it becomes a matter of importance to endeavour to bring the balance speedily to rest without, if possible, impairing its sensitiveness; as thus much time and labour would be saved in weighing. Several methods of applying gaseous friction for this purpose have been tried by me of late. By far the most successful consists in suspending from the beam, either within or beyond the scale-pans,

two very light closed cylinders which fit very closely (but without touching) into two fixed cylinders open at the top. Applied to a long and massive beam with considerable loads in the scale-pans, and which vibrated for some minutes when disturbed, this trial apparatus brought it to rest after, at most, *three* half vibrations. It is now evident that with a properly-constructed damper on this plan, there is practically no limit (so far as rapidity of weighing alone is concerned) to the length which may be given to a balance-beam; and, of course, no limit to the consequent sensibility of the instrument.

A very instructive hydrodynamical result was observed with this arrangement. The closed cylinder, exactly balanced inside the cylinder open at the top, is made to ascend briskly by a current of air blown even vertically downwards on the centre of its upper end.

b. Photographs of Electric Sparks taken in Cold and in Heated Air.

(*Ordered by the Council to be printed in the Transactions.*)

c. On the Electric Resistance of Iron at High Temperatures.

This note details various experiments made for me by Messrs C. M. Smith and A. Macfarlane in the Physical Laboratory of the University, and has been drawn up by these gentlemen. The only part I have taken in the work has been the suggestion of the line of investigation and the forms of apparatus employed. I mention this not alone in justice to them; but also as giving independent corroboration of results formerly arrived at by myself. [*This paper will be inserted later, when the necessary diagram is ready.*]

Monday, 1st March 1875.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Biographical Notice of William Euing, Esq., F.R.S.E. By Professor William P. Dickson.

William Euing was born on 20th May 1788, at Partick, where

his father had a printfield on the banks of the Kelvin. His family originally belonged to Strathendrick, which was, along with the Lennox, the chief seat of their name. Mr Euing conceived the latter (which he traced back to Domesday Book) to be connected with Eugenius, and was somewhat particular as to its being correctly spelt with a *u*. His grandfather settled in Glasgow about 1740, and was a magistrate of the city in 1767. His father went to the West Indies in 1799, and died there; whereupon Mr Euing, who was an only child, was left to the charge of his mother, and of his uncle, Archibald Smith of Jordanhill. This relationship laid the foundation of the close friendship that subsisted between Mr Euing and his cousins, Mr James Smith of Jordanhill, F.R.S.L. & E., and Mr William Smith of Carbeth-Guthrie (Lord Provost of Glasgow in 1823), during the prolonged lives of all the three. After receiving his elementary education at two private schools, Mr Euing was sent to the Grammar School, where he had as his class-fellows the late William Lockhart of Milton-Lockhart, M.P., his own cousin Mr Robertson Reid of Gallowflat, and other subsequently well known citizens of Glasgow, all of whom he survived. He entered the University in 1800 at the age of twelve, and attended the classes of Professors Richardson, Young, Jardine, and Millar. Although an earnest student, he did not complete the regular curriculum, but early entered on business in the calendering firm of Inglis, Euing, & Co., of which he soon became a partner. In 1815, in consequence of the work being too great for his delicate health (for at this period and down to 1845 he suffered much from illness) he retired, and, after acting for some years as a commission merchant, he began in 1819 the business of an underwriter and insurance broker, in which he continued at the head of the well-known firm of William Euing & Co.. till the close of his life, visiting daily almost to the last his office in the Exchange.

As a merchant, Mr Euing was held in the highest esteem by all who came into contact with him for his intelligence, his soundness of judgment, his probity, and stainless honour. He was a large shareholder in the unfortunate Western Bank, and its failure in 1857 brought into play at once his excellent habits of business and his cheerfulness of temper. He carefully and promptly arranged his own private affairs with a view to the worst, and then, as direct-

ing the proceedings of the Shareholders' Committee, applied all his energies to unravel the complicated affairs of the Bank, and to retrieve as far as possible the ruined fortunes of the shareholders —a task in which he was acknowledged to be beyond expectation successful. He was very methodical in his habits, one of which was early rising; and, long after he had reached eighty years, his elastic step might have been seen almost daily in the West End Park—a mile from his house—at an hour when but few were awake.

Mr Euing was in early life somewhat shy and reserved, having in his characteristic modesty formed a more humble estimate of his own abilities, and of his fitness to take part in society, than was entertained by those who had the privilege of knowing him in after years, of profiting by his varied information and refined taste, and of observing his deep and lively interest in literary and social questions. He early set himself to the task of self-improvement; his thirst for fresh knowledge never abated; and he found a constant pleasure in its gratification. His letters written from the Continent, during his last tour a few years ago, show, I am told, the same desire to learn everything, as do his letters written in 1816, when he made his first visit to France; and many of his books, even of those lately acquired, contain memoranda indicating their perusal and evincing a marked interest in their contents. In politics he took little part. Though earnest in his religious opinions, which were formed with conscientious independence and held with firmness, he was very tolerant in spirit; and, catholic in his sympathy with all forms of Christian work that approved themselves to his judgment by their fruits, he had but little relish for controversy. Simple and unostentatious in his personal habits, he yet found pleasure in the frequent exercise of a genial hospitality, to which his unfailing cheerfulness lent a special zest.

Mr Euing was eminently successful in business, and at full liberty—so far as family ties were concerned—to follow the bent of his own wishes and tastes in the application of his wealth. He had a singularly warm and generous heart, and was early drawn by it into those walks of practical philanthropy, with which his name is specially associated in the minds of his fellow-citizens, and in which he found growing pleasure as years went on. With rare self-denial he made it a rule—to which he systematically

adhered—to set apart a large proportion of his income to purposes of benevolence. Upwards of forty years ago he began to investigate the hardships connected with imprisonment for debt, and he took a zealous and important part in procuring their mitigation. Subsequently his sympathies were warmly enlisted on behalf of Sailors' Homes, and the thriving Home in Glasgow, which was to a very large extent erected by his liberality, was the object of his constant care and unwearied bounty down to the close of his life. He bequeathed to it a legacy of L.2000; and a bust, from the chisel of Mr G. H. Ewing, has just been placed by the Directors in the hall of the institution as a fitting memorial of its patron. Almost all the public charities of Glasgow received, in addition to his regular contributions, special proofs from time to time of his liberality; and equally cordial was his interest in the Bible Society, the City Mission, and other schemes to promote the good of the community.

Not less remarkable was his interest in education, science, and art. Not to speak of his services and benefactions to the Buchanan Institution, the Mechanics' Institute, the School of Art, Stirling's Library, the Botanic Garden (to which he left L.3000), and other agencies for helping the education and elevating the tastes of the people, he manifested a specially warm and constant zeal for the prosperity of Anderson's University, of which he was long the most valued counsellor and, along with his friend Mr Young of Kelly, the most conspicuous and munificent patron. He devoted much of his time to its service, cherished a lively interest in its work and in its teachers, repeatedly made large donations to its funds, and, besides founding and endowing in it a Lectureship on Music, left to it a legacy of L.6000. He was an early subscriber of L.1000 to the new buildings of the University of Glasgow; and, besides various donations during his life, he has destined the sum of L.6000 to the endowment of fellowships bearing his name, the holders of which are to conduct tutorial classes of limited numbers, more especially during the vacation, and thereby "to confer on the University some of the benefits derived from tutorial instruction at the English Universities." Mr Euing was also a liberal patron of art, and had formed a considerable collection of pictures, thirty-six of which he presented during his life to the Corporation of

Glasgow. He has now bequeathed to them the remainder, giving powers of sale or exchange, but directing the retention of at least fifty of his pictures in their gallery. His refined taste was visible also in a collection of old silver plate and china. Mr Euing was a Fellow of the Scotch Antiquarian Society, and President of the Glasgow Archæological Society. He was long an active member, and one of the last surviving councillors, of the Maitland Club, to which he presented a volume prepared by the Rev. Mr Muir of Dysart, containing extracts from the records of that burgh. He was ever ready to countenance and encourage any apparently meritorious enterprise of antiquarian authorship. He had formed a very remarkable and highly interesting collection of autographs, which, as his will does not indicate any special destination for it, will probably fall to be dispersed.

But, of all the noble forms which the gratification of his personal tastes assumed, that on which he bestowed most attention, and which he valued most—cherishing in it a peculiar modest pride—was his library. It consisted of three distinct divisions. The first contained a very large and—so far as Scotland at least is concerned—unrivalled collection of music and of works on music, amounting to several thousand volumes. Mr Euing was an enthusiast in music, and was conversant alike with its theory and practice; indeed his love for it was so intense, that in early life he was in the habit of meeting with some friends of similar tastes as a Saint Cecilia Society at, I think, five o'clock in the morning. This musical library is bequeathed to Anderson's University in connection with the Euing Lectureship in Music, along with L.1000 towards providing a fireproof apartment for its reception, and L.200 for the compiling and printing of a catalogue. The second division consisted of a still larger and invaluable collection of editions of the Bible and its parts, chiefly of the various English versions (which are very largely represented), but including also a very considerable number of Polyglott, Greek, Latin, French, German, and Dutch Bibles, and not a few in other languages, along with numerous Psalters, and Books of Prayers and Hymns, amounting to nearly 3000 volumes. This has been left to the University Library, to be retained as a special collection. The third division was his general library, amounting to nearly 20,000 volumes, which

is also bequeathed, with a few exceptions, to the University Library. This miscellaneous collection possesses many features of interest. It embraces about 150 volumes printed before A.D. 1500, special collections of works printed by the Aldine, Stephanic, and Elzevirian presses, of books printed at Edinburgh, Glasgow, Aberdeen, &c., in the 17th century, as well as of Baskerville, Barbou, Bodoni, Didot, Urie, and Foulis classics (those of the Bodoni and Foulis presses being especially numerous), the first and second folios of Shakespeare and many rare first editions of English classics; a large number of privately printed works (including a great many of the reprints issued in very limited number by his friend Mr Halliwell-Phillips) numerous books on bibliography, archæology, and the fine arts, an extensive series of English minor poems, ballads, and songs; a very curious and unique collection of Broadsides, and a few MSS., including a Hebrew Roll of Genesis of great beauty. The books have been selected by Mr Euing with much care and judgment; many of them are large paper copies, or present other specialities of bibliographic interest; and most of them are tastefully bound. The value of this gift to the University cannot be estimated at less than L.10,000. Mr Euing has judiciously empowered the Senate to sell duplicates to the extent of half his general collection; and has directed the proceeds to be applied towards the maintenance and binding of the collection, or the purchase of other books to be added to it.

Mr Euing died on the 16th May 1874, closing, in the words of a relative, "gently and without suffering a long and useful life, and not leaving a single enemy." He was a Glasgow merchant of the noblest type. Others may have equalled him in the shrewdness and worth, or in the generous heart and open hand, which happily are no uncommon characteristics of the order to which he belonged; and some, of ampler resources, may even have surpassed him in the success with which they have made their wealth minister to the gratification of some particular taste; but in the combination of the highest standing as a merchant with the zeal of a philanthropist and the refinement of a connoisseur, in the many-sided excellencies of his character and the variety of his literary and artistic taste, and in the wise destination of his resources alike during life and at death, Mr Euing may well be regarded as unique.

2. On a Faulty Construction common in Skewed Arches. By Edward Sang, Esq.

In the course of engineering works, bridges have to be thrown obliquely over rivers or over roads, and thus the construction of oblique or skewed arches is forced upon us. The skewed stone arch has not grown in favour, partly from the greater skill required in delineating and executing the forms, partly from the fact that such skewed arches have given signs of weakness. Hence an impression has gained ground that there is something defective in the principle.

The defect, however, of those skewed arches which I have seen lies entirely in an erroneous mode of construction, which, but for the cohesion of the lime, would result in an immediate downfall. The pervading principle of all good mason work is this: that the surfaces of each stone should be dressed square to the pressures transmitted by them. Now, along the ridge or centre-line of the skew this principle is attended to; the beds of the arch-stones are placed square to the line of the roadway, that is to the line of pressure there. In consequence the line of the course begins to descend on the surface of the vault; and, in continuing the descent, the architect lays off equal distances on the curves to correspond with equal distances along the crown. Hence all the courses present equal breadths measured along the lines of pressure.

The inevitable consequence of this arrangement is, that the bed of the stone becomes more and more oblique to the pressure as we come down on the haunch of the arch; the mechanical effect being just the same as if a mason, in building a wall, were to place the stones off the level. The ends of the stones, as seen on the plane of the parapet, present, in this case, equal graduations, and whenever we see the ends of the arch-stones equally placed, we may be sure that this fault pervades the whole structure. The fault is clearly seen on one side of the model of a skewed centering exhibited to the Society.

Beginning at the crown of the arch, and descending in this way, the course becomes inclined to the line of pressure; it is necessary to bend it gradually upwards from the course just described, and

the problem becomes this,—"To draw upon the surface of the vault a curve which shall cross all the lines of pressure squarely." This belongs to a well-known class of problems in what is called the calculus of variations.

The nature of this curve must depend on the character of the arch; yet it has certain general features independent of that character. The chief of these may be explained in this way. If we take two closely contiguous curves, inclosing between them, as it were, a course of arch-stones, the breadth of that course, at any place, is proportional to the cosine of the inclination of the line of pressure. Hence, in every skewed arch the breadths of the stones as seen on the parapet plane, must diminish from the crown downwards, becoming at 60° from the crown just half as broad as at the top.

In the case of the circular arch, the projection of the curve upon the plane of the parapet is the well-known tractory, which is asymptotical to the horizontal line passing through the centre. Hence we cannot have a semicylindric skewed arch, because the curve of the course-joint cannot reach to the vertical part of the surface.

The nature of the true arrangement is shown on the other side of the model.

A glance at the ends of the arch-stones of any skewed bridge is enough to apprise us of whether or not the structure have been properly arranged.

3. On the mode of Growth and Increase amongst the Corals of the Palæozoic Period. By H. Alleyne Nicholson, M.D., D.Sc., Professor of Biology in the Durham University College of Physical Science.

In the first portion of this communication, the author discussed the general phenomena exhibited by the Palæozoic corals as regards their mode of growth and increase. Five chief modes of growth were distinguished:—

a. Simple calicular gemmation, in which the corallum sends up from its calicine disc a single bud, which usually repeats the process, until there is produced a succession of corallites vertically superimposed upon one another. The peculiarity of this process consists in the fact that the same calice never produces more than one bud.

b. Compound calicular gemmation, in which the primitive corallite throws up two or more buds from its oral disc, these in turn usually repeating the process, till the corallum comes to form an inverted pyramidal mass, composed of numerous corallites diverging from the base.

c. Basal or marginal gemmation, in which new corallites are produced at the circumference of the colony or along certain definite lines proceeding from the base.

d. Parietal or lateral gemmation, in which the increase of the corallum is by the production of buds at some point in the walls of the parent corallite between the lip of the calice and the base.

e. Fission, in which the growth of the corallum is effected by the cleavage of the calice of the original corallite or corallites.

Numerous examples were adduced of the occurrence of the above modes of growth, singly or in combination, amongst the Palæozoic corals, and various modifications of these processes were discussed.

The author next discussed the value of the mode of growth of the corallum as applied to the classification of the Palæozic corals, and arrived at the conclusion that much stress could not be laid upon this point unless accompanied by other distinctive characters as well. The chief grounds upon which this conclusion was based were, that allied forms in the same genus, and sometimes even different individuals in the same species, show entirely different modes of growth; that forms belonging to the most remotely allied groups often increase in the same way; and finally, that the difficulty in determining the precise mode of growth amongst some of the fossil corals is so great as often to render this test practically inapplicable.

In conclusion, the author discussed the relations between the growth of the different parts which may comprise a compound corallum, as regulating its final form and structure.

4. The President exhibited Diagrams in illustration of the Capillary Surfaces of Revolution.

The following Gentlemen were duly elected Fellows of Society:—

CHARLES WILSON VINCENT, Esq., London.
RALPH RICHARDSON, Esq.
JOHN RAMSAY L'AMY, Esq.
E. W. PREVOST, Ph.D.
JAMES SYME, Esq.
Sir JOHN HAWKSHAW, F.R.S.

The following Gentlemen were duly elected Foreign Honorary Fellows:—

HEINRICH WILHELM DOVÉ, Berlin.
AUGUST KEKULE, Bonn.
HERMAN KOLBE, Leipzig.
ERNST EDUARD KUMMER, Berlin.
JOSEPH LIOUVILLE, Paris.
JOHN LOTHROP MOTLEY, U.S.

Monday, 15th March 1875.

DAVID MILNE HOME, Esq., LL.D., Vice-President, in the Chair.

The Council having awarded the Makdougall Brisbane Prize for the Biennial Period, 1872–74, to Professor LISTER, for his paper "On the Germ Theory of Putrefaction and other Fermentative Changes," Dr Crum Brown, in requesting the Chairman to present the medal, addressed the Chairman as follows:—

MR CHAIRMAN,—I have been requested by the Council, and I feel it a very great honour that I have been so requested, on the occasion of the presentation of the Makdougall Brisbane prize to Professor Lister, to state shortly the grounds upon which the Council have made the award.

Every Fellow of the Society who had the privilege of hearing Professor Lister's paper read, must have a vivid recollection of the interesting and admirably clear manner in which he explained an intricate series of experiments; of his hereditary ingenuity

in devising and skill in carrying out delicate mechanical contrivances, and of the eloquent as well as cogent logic with which he enforced his conclusions. I wish it had fallen to one more fit to do justice to the subject, to lay before the Society an abstract or summary of this very remarkable paper.

Professor Lister's work may be considered from several different points of view.

I. As a contribution to microscopic botany, and as such it takes a very high place. A great obstacle in the way of the study of microscopic plants is the difficulty of the determination of species. Each species is liable to great variation in form, and there is a great general resemblance between forms assumed by different species. To get over this difficulty, the method of "cultivation" has been made use of—the doubtful specimen is kept and grown to see what it will become. Professor Lister in this paper describes his novel method of cultivation, in which the fungi are made to grow in various kinds of soil. Thus, two fungi growing in Pasteur's solution may resemble one another very closely; but if transplanted into milk, and allowed to grow there, a very marked difference may be produced. Or two fungi may present in one solution forms indistinguishable from one another, but one may grow luxuriantly and the other not at all, when transferred to a different solution. Such cultivation experiments are apt to fail from a character which they have in common with cultivation experiments on a larger scale. The miniature garden, like other gardens, is liable to be infected with weeds, and it sometimes happens that such a weed, or unwelcome intruder, is mistaken for the produce of the seed sown or the plant planted. These weeds grow either from seeds contained in the soil, or introduced from without, and it is essential to a successful experiment that the first be killed or removed, and the second excluded. Professor Lister secures the necessary condition of purity of the soil, perfect freedom of his solutions from all trace of life except those fungi or germs purposely introduced, and perfect security against accidental or unintentional entrance of any living thing, without interfering with the readiness of access to each experiment during its progress. This is accomplished by means of devices, of which it is difficult to say whether the complete success or the wonderful simplicity is more striking. The

results obtained need not disappoint the most sanguine investigator. Professor Lister has obtained proof that Bacteria are, at all events in some cases, directly derived from fungi, of which they are merely a special development. He has been able to determine, within not very wide limits, the number of individual germs contained in a drop of water, and to show that this number is greater in warm than in cold weather, and has proved that the number of distinct species of microscopic fungi is great beyond all previous imagination. There is one special result to which I cannot omit a reference. It is, that there are certain fungi which, although rare, and, we may therefore conclude, not, under ordinary conditions, hardy, still flourish luxuriantly and increase rapidly under certain special conditions. Thus the fungus which causes the lactic fermentation of sugar, is scarcely to be found anywhere but in dairies. Boiled milk or perfectly pure milk, may be exposed to air anywhere else without undergoing the lactic fermentation; other fungi, producing different effects, will grow in it; but if milk be exposed in a dairy. this particular fungus overcomes all others, and the lactic fermentation alone takes place. Milk is the soil specially suited for its growth, but it does not appear there of itself—it must be introduced from without.

II. Another matter of great interest connected with Mr Lister's work, is the means which it will no doubt put into our hands of preparing many chemical substances. Although he has not fully investigated the various chemical changes which accompany the growth of microscopic fungi, he has shown that each species produces its own effect; and as he has taught us how to obtain specimens of each species without mixture of any other, he has put it in our power to produce specific fermentations, and study them undisturbed by the presence of other kinds of fermentation.

III. But more general interest attaches to Mr Lister's paper as a very important step in the settlement of the question: Does life ever arise from lifeless matter, or is the origin of life not as great a mystery and as far removed from the grasp of our scientific methods as the origin of matter itself? If living things never develope out of dead nature in the ordinary processes of nature, we are forced to the conclusion that they either have existed always, or have been miraculously created. It has been sup-

posed that there is a logical difficulty in the way of proving that life does not grow out of dead matter—that to attempt to prove this is to attempt to prove a negative. But every man of science believes that the quantity of matter is constant, and that the quantity of energy is constant, although these propositions equally involve the negatives, that matter and energy never appear or disappear, but merely undergo transformations. But although there is no absurdity, there is a great difficulty in the way of proving that living beings are always produced from pre-existing living beings. It is difficult to make our experiments under precisely the conditions under which nature works, and at the same time to exclude the possibility of the presence of living beings. If we boil our liquid in order to kill its living contents, it may be said that we change its chemical character, and deprive it of the power of producing life; if we shut it up in a hermetically closed vessel, we prevent that access of air which *may* be essential to the production of life from lifeless matter. Mr Lister has shown us how we may obtain milk, urine, and blood quite free from living beings, and keep them liquid for any length of time freely exposed to air without any risk of the entrance of living things, and he has shown us that if this be done no living things ever appear in the liquid. In his experiments we see two samples of the same substance treated, with one exception, in exactly the same way; in the one sample life is abundantly developed, in the other not at all. Can any reasonable man doubt that this striking difference of result is due to the one only difference of treatment; and this difference of treatment is merely that in the one case living things have had access to the substances, in the other they have been excluded? In all other respects the two samples have been exposed to precisely the same influences. With all respect for those experimenters who, not having taken Mr Lister's precautions, have arrived at different results, I express my conviction that it has been definitely proved that life is continuous, that living matter cannot be produced by a chemical process, and that every living thing is descended from some previously existing living parent.

IV. Another aspect of this paper is of less general scientific interest, but of much greater practical importance. Mr Lister's investigation forms the scientific basis of the system of antiseptic surgery, with which his name will always be associated. The

microscopic fungi, in the consideration of which we have been engaged, perform a very important function in nature. They form a brigade in nature's army of scavengers. They transform the dead matter which once formed part of organised living beings into raw materials out of which new organisms construct their bodies; they break down the complex substances, when the complexity has become useless, into simpler compounds which can be used again. They demolish the old ruins, and render their stones fit to be employed as building materials. But they not only attack the dead, they kill the weak and the dying; and while this action may be considered useful on the whole, as leaving room for the development of the strong, it is precisely the duty of the medical man to combat this tendency of nature, to support the weak that it may have an opportunity to become strong, to ward off nature's blows that the dying may recover. This is not the place to speak of the extraordinary results obtained by Mr Lister's mode of treatment, of the certainty of cure in cases which ten years ago would have been considered absolutely hopeless; my object is rather, assuming these results, to show how intimately they are connected with the scientific truths which form the basis of this mode of treatment.

It has been suggested, and I confess that I at one time thought the suggestion a good one, that instead of trying to convince surgeons of the truth of the scientific basis, Mr Lister should draw up a code of practical rules which a surgeon might follow without thinking of germs or bacteria or fungi. A little consideration will show the absurdity of this idea. A surgeon impressed with the truth of the scientific basis needs no code of rules—he sees at once what he must do, and what he must avoid. A code of rules drawn up for one ignorant of the scientific basis would be intolerably complicated, and certain to be violated. In this, as in other and higher and more general motives, faith is essential to practice: if we know the why, we can, as a rule, find out the how; and antiseptic surgery will be successful then, and then only, when the reasons for its methods are understood and believed in.

I have endeavoured, Sir, to lay before the Society some of the reasons which have led the Council to award the Makdougall Brisbane prize to Mr Lister, and I hope I have in some measure succeeded. I cannot express the satisfaction we all feel in having a paper so eminently worthy of the award.

The following Communications were read:—

1. On the Diurnal Oscillations of the Barometer. By Alexander Buchan, M.A.

2. The Phenomena of Single and Double Vision, as shown in the Stereoscope. By R. S. Wyld, Esq.

When we direct the axes of the two eyes to any definite object, its different parts affect *corresponding* parts of each retina, and the object appears *single*. When we squint, or do not look direct at the object, its images affect *non-corresponding* parts of the two retinæ, and the object appears double. The more widely the axes of the eyes are deflected from the object, the further asunder the double objects seem; and the less the axes are deflected the less distant from each other the double objects appear. Thus, when we hold the finger in front of the eyes while we look at a distant candle or gas jet, the flame appears single and the finger double. When we turn the eyes to the finger it appears single and the flame appears double.

The paper read to the Society in February 1871 was an attempt to prove that all the phenomena connected with single and double vision were explanable on the supposition that the nerve fibres of each retina decussate in a common cerebral sensorium, as for instance in the *corpus quadrigeminum*, which the optic nerves are known to enter; and that as, owing to the fineness of the texture, anatomists had hitherto been unable to determine the ultimate arrangement of the fibres in the brain, we were justified in making this suggestion.

Such a crossing of nerve fibres has in it nothing improbable, for there are many instances of such crossings, as for instance in the great and in the lesser commissures of the brain. There is also a similar crossing in the *medulla oblongata* of the motor nerves from the brain, before they descend the spinal cord; and there is a similar crossing of the sensory nerves where they enter the spinal cord. The supposition then of a decussation of the fibres of the optic nerve within the brain is in analogy with what we know to be of frequent occurrence in the body.

The facts which Mr Wyld now brings before the Society are these—

1st, When we enter two slips of white card-board, one at each side of the stereoscope, they affect *non-corresponding* parts of the retinæ, as shown in the diagram exhibited, and they appear as *two objects*.

2d, When we push the slips forward till they appear to overlap, the overlapping ends appear as one object, because they affect the corresponding central points of the two retinæ. If we make a mark similar in form and size on each slip, but do not approach the slips sufficiently near for the marks at once to coalesce, such marks are nevertheless frequently observed to glide closer and coalesce. This is owing to the natural tendency we have to direct the axes of the eyes to the objects examined. This causes the marks to affect corresponding points of the retinæ, and the marks consequently coalesce visually and appear as one object.

3d, When the slips seem to overlap, the overlapping portion appears so greatly increased in brightness that the other parts have a tendency either to disappear altogether, or they appear and disappear at brief intervals, so long as we continue to look at the central bright portion. These dim or invisable outlying parts may, however, at any time be made to reappear by simply moving the card-board once or twice up and down, and thereby exciting the attention and the retina. They may also be made to appear by winking, by moving the eyes from side to side, or doing anything to stimulate the retinæ.

4th, With regard, again, to the overlapping parts, it is to be observed, that though they appear at first sight to form, as it were, one single object, yet it is easy to see that this bright part is in reality a double picture containing the impression received from each eye; and so far as these impressions are not incompatible, but may be blended the one with the other, they go to form as it were a composite picture, as we know is the case with the figures on the usual stereoscopic slides, and as we may prove to be the case by making any distinctive marks on the slips of card-board, when these marks will appear distinctly visible, as if integral parts of the overlapping portion, though seen by the different eyes.

5th, Another important circumstance is this. When the slips are of different colours, as for instance one slip red and the other blue, or one blue and the other yellow, these colours, when caused to overlap in the stereoscope, do not produce the intermediate

colours of purple or green; on the contrary, as was stated in the paper alluded to, at one time the coloured slips appear alternately visible, at another time one half of each may be visible, and occasionally, spots, or it may be only minute specks, smaller even than the fifth part the diameter of a small pin head of the one colour, will be seen shining on the ground colour of the other card-board. These particular changes seem to depend greatly on the excited or the fatigued condition of the retinæ at the time; for if we direct our attention to any conventional mark made on either of the slips presented, the excitement of the retina of the eye, caused by the act of observing the mark, immediately causes the slip on which the mark is made to become visible, and the mark appears surrounded with a patch of the colour of the slip on which it is placed.

Two circumstances then may be mentioned as certain: that in no instance do the two colours blend into an intermediate colour; and second, that we never observe the same portion of the bright overlapping portion to have at one and the same moment two different colours; parts or spots or minute specks may, as we have said, appear of the one colour, and other parts may appear of the other colour, but though the one coloured slip visually overlaps the other differently coloured slip, we never see any part at once to possess two different colours.

The conclusions to which these phenomena lead are certainly these—that there is a physical union in a cerebral lobe of the nerve impressions coming from the two eyes, and in no other way can we account for the two retinal images giving the mind the impression of but one object both in natural and stereoscopic vision when corresponding retinal fibres are excited, and of double objects when non-corresponding fibres are excited—and no other supposition will account for the increased brightness obtained by the use of two eyes than that suggested, namely, that the nerve impressions from both eyes are physically united in the sensorium.

Another conclusion to which we are led is, that though the corresponding retinal fibres are brought into juxtaposition in the sensory, yet they are not there joined or amalgamated the one with the other, seeing they do not produce the effect of an intermediate colour, but each fibre transmits to the sensory the distinctive colour and impression which it receives in the retina.

The reason why we never see any one part of the overlapping stereoscopic objects simultaneously exhibiting either two different colours, or an intermediate colour, is a matter more difficult to explain; perhaps the following may be considered sufficient explanation. If the *smallest visible point* is a point due to the impression produced by a *single nerve fibre* from one of the eyes, then, as on the supposition of a decussation of the fibres in the sensory alternate, exceedingly small specks of different colours may at any time appear intermixed, from the circumstance of the supposed *alternate juxta position* of the individual fibres from each retina in the sensory, so, if this supposition is correct, it is evidently impossible that we can ever have the impression of two different colours superimposed on the same point and at the same moment of time.

3. On the Products of the Oxidation of Dimethyl-Thetine, and its Derivatives. By Prof. Crum Brown and Dr E. A. Letts.

The difficulty experienced in determining the sulphur contained in the compounds of *dimethyl-thetine* (described in a former communication) by oxidation to sulphuric acid, induced the authors to study the effects of various oxidising agents on these compounds.

By acting on *nitrate of dimethyl-thetine* with *dilute* nitric acid, two bodies are produced. The one has acid properties, and forms a well-marked soluble salt with baryta. The other has neither acid nor basic properties. It crystallises in very beautiful needles from a hot solution in alcohol.

By acting on the base *dimethyl-thetine* with permanganate of potash solution, the same crystalline substance is produced, but the presence of the acid body could not be ascertained. The oxidation of *dimethyl-thetine* by permanganate of potash takes place in acid or alkaline solution and in the cold.

Chromic acid has no action whatever on *dimethyl-thetine* further than combining with it to form *chromate of dimethyl-thetine*—a yellow gummy substance which refuses to crystallise. The same body may be produced by acting on a solution of hydrobromate of dimethyl-thetine with chromate of silver.

Fuming nitric acid dissolves solid *hydrobromate of dimethyl-thetine* without rise of temperature, but with separation of bromine.

On warming the solution, brisk action ensues. When this has terminated, and the nitric acid has been evaporated off on a water bath, a strongly acid syrup remains, which fumes like hot sulphuric acid. This syrup also forms a soluble barium salt.

The investigation of the compounds produced by the oxidation of dimethyl-thetine and its derivatives is proceeding, and the authors trust in a short time to be able to communicate the result of their experiments to the Society.

Monday, 5th April 1875.

Professor KELLAND, Vice-President, in the Chair.

The Council having awarded the Neill Prize for the Triennial Period, 1871–74, to Mr Charles William Peach, for his contributions to Scottish Zoology and Geology, and for his recent contributions to Fossil Botany, Professor Geikie, on the presentation of the medal, addressed the President as follows:—

Sir,—The Council of the Royal Society of Edinburgh has awarded the Neill Prize for the triennial period, 1871–74, to Mr Charles William Peach, and on the part of the Council I am requested to describe briefly to the Society on the present occasion the nature of his scientific work, which has been judged well deserving of one of the Society's medals. By the terms of the original bequest this prize is reserved for the work of a Scottish naturalist. Born in Northamptonshire, Mr Peach might seem to be excluded from the list of those to whom the prize can properly be awarded. But for more than a quarter of a century he has lived continuously in Scotland, and during that time has done at least as much as any living Scotsman to extend our knowledge of the natural history of his adopted country. From the Kyles of Sutherland to the holms of Roxburgh, he has never resided in or near any Scottish county without adding something to what was previously known about its flora and fauna, either living or fossil. The Neill bequest likewise provides that the paper or work for which the prize is given shall bear date within five years previous to the award. During the last five years Mr Peach has contributed some valuable materials towards the extension of our knowledge of the fossil plants and fishes

of the Carboniferous rocks of the basin of the Forth. But the Council has considered that it will best conform to the liberal spirit of the founder, Dr Neill, himself, by having regard not only to Mr Peach's work during the last five years, but to all his labours in Scotland, which have so frequently aided the researches of his brother naturalists, from whom, in his old age, every token of grateful appreciation and kindly feeling is justly due.

The naturalist, not less than the poet, is born, not made. The quickness of eye which, without effort, lets nothing escape notice; the fine instinct which divines the meaning of half-hidden phenomena, and leads on to where further successful observations should be made; the patience with which repeated failure is borne; the enthusiasm which, amid foul weather or fair, brings the observer back joyously from the cares of this world to his self-chosen task, whether it be among the treasures of land or of sea,—these are qualities which no education can supply to us, and which no want of education can wholly repress. Mr Peach has been happy in the possession of them to no common degree. Appointed more than half a century ago to the Coast Guard Service, and necessarily restricted in his pursuits by the arduous duties of that calling, he has everywhere during that extended period shown the genuine characteristics of the born naturalist. His enforced residence near the sea has been turned by him to excellent account, for he has materially increased our acquaintance with the marine fauna which surrounds our islands.

Somewhere about twenty species, and several genera of sponges, were first made known by him as denizens of British seas. He has considerably augmented our list of native hydrozoa and polyzoa. The naked-eyed Medusæ owe not a little to his attention, and one genus of them (*Staurophora*) was first introduced by him to the naturalists of this country. The Echinoderms, too, are under similar obligations to him, for besides bringing several new species to light, he found the huge *Echinus melo* of the Mediterranean on the coast of Cornwall, and supplied the twenty-armed *Holothuria nigra* to fill up the blank pointed out by Edward Forbes among the British Holothuriæ.

Since his removal to Scotland in 1849, Mr Peach has done further and most valuable work among the mollusca and fishes, adding to our fauna several species of shell as well as some fishes—Yarrell's

Blenny, Ray's Bream, and the Anchovy, for example—which were not before known to occur so far north as the seas which wash the northern shores of Scotland. In none of his labours does the true spirit of the naturalist appear more pre-eminently than in those by which he made known the nest-building habits of certain sea-shells and fishes. At Wick he noticed that the jelly-like masses of the ascidian *Leptoclinum* very frequently contained small yellow patches in the centre. Watching these, he found that the central yellow parts were really extraneous bodies, and consisted of nests containing ova. Further observation connected these ova with the slug-like gasteropod *Lamellaria*, and showed him that this shell comes every spring regularly to shore from deeper water outside, and remains two or three months for the purpose of nidification. Again, at Peterhead he made himself intimately acquainted with the family arrangements of that rather fierce-looking little fish, the 15-spined stickle-back (*Gasterosteus spinaceus*). In a rocky pool he found a colony of them, and learnt how they built their nests and deposited their ova. He watched the hatching and growth of the young until the whole colony, young and old, took to the sea. As he used to visit them five or six times a day, the parents grew so familiar that they would swim round and touch his hand, though on the appearance of a stranger they would angrily dash at any stick or incautious finger that was brought near them. The same habit of close and cultivated observation was shown by his study of the maternal instincts of the female lobster in its native haunts.

Previous to Mr Peach's transference to Wick, very little was known about the fossil plants of the Old Red Sandstone of Caithness. Many specimens had been found, but they were commonly spoken of as indistinctly preserved, and as probably of marine origin. Setting to work among the dark flagstones of that district, he eventually succeeded in forming an admirable collection, and in showing the truly terrestrial nature of that ancient flora. Within the last few years he has continued his services to fossil botany by bringing to light some new and most interesting vegetable forms from the Carboniferons strata of the basin of the Forth. He has shown, for example, the connection between the flower-like *Antholites* and the usually detached fruit, *Cardrocarpon*, and has obtained in one fossil a conjunction of microspores and macrospores.

To palæontology he has contributed several new species of fishes from the Old Red Sandstone and the Carboniferous rocks. In great measure to his perseverance do we owe our present list of the ichthyolites of Caithness and the Orkney Islands. But perhaps the most important item of his labours, in this department, at least, if we regard questions alike in theoretical geology and in the geological structure of Britain, was the discovery of fossils in the limestones of Sutherland. Previous to his observations the rocks of the Scottish Highlands were usually grouped with the so-called "Azoic" rocks, as if they belonged to a time anterior to any of the fossil-bearing formations of the country. Obscure organic remains had been indeed detected many years before by Macculloch in the quartz-rocks of Sutherland, and these were afterwards brought again into notice by Hay Cunningham. But they had gradually passed out of mind, their organic nature being stoutly denied even by such geologists as Sedgwick and Murchison. Mr Peach, however, brought to light a good series of recognisable shells and corals, which demonstrated the limestones containing them to lie on the same geological horizon as some part of the great Lower Silurian formations of other regions. It was this discovery which enabled Sir Roderick Murchison to clear up the geological structure of the Highlands, and entitled him to be the first Brisbane medallist of this Society.

In every department of natural science to which Mr Peach has given his attention he has distinguished himself as a keen-eyed and enthusiastic collector, with an almost unrivalled shrewdness in detecting what was new, and at the same time with a disinterested readiness to hand over his materials to those who had more specially studied the department of natural history to which these materials belonged. For his varied contributions to science, carried on for so long a time, with a purity of motive and a generous helpfulness towards others which have won for him the esteem of all naturalists, and with an enthusiasm which the lapse of more than threescore years and ten has left undimmed, the Council has adjudged to him the Neill prize. I beg on their part to present him to you, with the cordial wish that he may yet live for many years among us as an honoured type of the true collector and naturalist.

The following Communications were read:—

1. On the Physiological Action of Light. Part II. By James Dewar, Esq., and Dr John G. M'Kendrick.

2. On the Structure and Systematic Position of *Tristichopterus alatus*, Egerton. By R. H. Traquair, M.D., F.G.S.

The cranial osteology and the dentition of *Tristichopterus* have been hitherto entirely unknown, and we were but imperfectly acquainted with the structure of the pectoral fins. Consequently great doubts have prevailed with regard to its affinities, though it was supposed to be allied in many respects to *Dipterus*. A suite of specimens from John O'Groat's, in the Edinburgh Museum of Science, collected by Mr Peach, the original discoverer of the fish, subsequent to the publication of Sir Philip Egerton's description, throws great light on the previously unknown points of its structure, as well as on its affinities. In the osteology of the head it presents a striking resemblance to the *Saurodipterini*, and to the genus *Gyroptychius*, as described by Pander. The teeth are acutely conical, and of two sizes, large and small; the larger teeth have their bases fluted externally, and internally the dentine is seen to be thrown into a series of simple folds, the pulp cavity becoming simple towards the apex of the tooth. The shoulder girdle is provided with interclaviculars; the pectoral fin is subacutely lobate. The structure of the head, the dentition, and the form of the paired fins, show that *Tristichopterus* has nothing whatever to do with *Dipterus*. It seems to be more closely allied to *Gyroptychius* than to any other known genus.

Tho following Gentleman was duly elected a Fellow of the Society:—

John Aitken, Esq., Darroch, Falkirk.

Monday, 19*th April* 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Note of Temperature Measurements in the Great Geysir of Iceland—August, 1874. By Robert Walker, Esq.

I have thought it might be of interest to the Society to lay before it a short account of some temperature observations which I succeeded in making at the Great Geysir of Iceland, in the month of August last year. As the circumstances of my visit to the island obliged me to limit my stay at that remarkable spring to a few hours, and as, during that time and for 48 hours previously, no great eruption occurred, I fear my results must appear somewhat meagre and unsatisfactory. The very interesting nature, however, of the problem of the action of the Great Geysir, and the difficulty of securing any reliable observations at all in so inaccessible a region, will perhaps be deemed sufficient grounds for my taking up the time of the Society with these few remarks. So far as my results go, they confirm very remarkably those of Professor R. Bunsen, who, with a companion, spent more than ten days at the spot in July 1846, and to whom science is indebted for the now generally received theory of the action of the Great Geysir. An account of his observations was given in the "Annalen" for 1847, and to it I shall refer frequently in the course of these remarks.

If the difficulty of obtaining thermometer readings at various points in a column of water from 70 to 80 feet deep, and more or less in a constant state of agitation, be great, the difficulty of reaching the place at all I found to be by no means inconsiderable. Arriving in Reykjavik on the afternoon of Monday, 3d August, after a very stormy passage of nearly five days from the Clyde, we experienced, owing to the visit of His Majesty the King of Denmark, more than the usual trouble and delay in securing guides and ponies, for the long ride of nearly 80 miles up country. It is

usual to take this in two stages, halting at Thingvalla, which is rather less than half way, or about 35 miles from Reykjavik. Starting with several fellow-travellers on the afternoon of Tuesday, the 4th, under orders to return to our steamer on Saturday evening, it was early morning next day when we pitched our tents on that classic plain where, on the Friday, the King was to address the assembled deputies from all Iceland. My companions decided to remain and witness this great national demonstration. After a great deal of trouble I at last succeeded in finding a native who had no scruple on patriotic motives to absent himself, and act as my guide on to Geysir; and, through the goodness of some Iceland friends, I was able to secure the companionship of a most intelligent lad of only 15 years of age, a student at Reykjavik, who, besides knowing Icelandic and Danish, could speak English remarkably well. Making an early start, then, with these two, and five horses, on the morning of Thursday, 6th August, we managed to reach the Geysir in 8 hours, meeting the king and his retinue on their way down. From one or two members of the American party, and some English travellers, who had preceded us, I learned that there were great expectations that the Geysir was at last to go off. It had erupted twice on the morning of Tuesday the 4th, but not since then, so that the king had been obliged to return, after boiling an egg at the edge of the basin. By those who were now about to follow him I was congratulated as being quite certain to see an eruption before morning; but no such good fortune was in store for us. Dr Hayes, of Arctic fame, kindly assisted in arranging the tackling of rope and cord which I had brought with me for letting down the thermometer, and one of the English party, a Cambridge man, the Rev. E. MacCarthy of King Edward's School, Birmingham, was even so good as to volunteer to remain behind and help me with my observations, an offer which I gladly accepted. I need hardly attempt to describe what travellers have so often described already, I mean the general situation and form of the Geysir tumulus, and the beautiful basin filled with pellucid water, by which this mound of deposited silica is crowned. The water in the centre seemed three or feet deep to the mouth of the funnel proper; but of course our measurements were necessarily taken from its surface in the basin, and this may account for the fact

that my measurement of the depth of the funnel is 3 or 4 feet in excess of Bunsen's.

In devising the apparatus employed I was kindly assisted by my friend Professor Fuller of Aberdeen. I procured from Casella of London a self-registering maximum thermometer, which is now on the table. We had made for it a case of brass, the ends of which were made to unscrew, and were pierced with holes. The thermometer was kept in its place in this case by little wedges of cork, which, however, allowed the water to have free passage through the tube from end to end. To protect the thermometer and case against injury from the sides of the Geysir-funnel, we slid over its ends two large pieces of cork, and connected these lengthwise with slips of wood. This arrangement was found to answer admirably, unless that the large masses of cork required so heavy a weight to submerge the whole apparatus that we had some trouble in hauling it in towards the side when each reading was taken. We had also omitted to provide swivels to prevent twisting of the cord when the cap was unscrewed, and from this cause much time was lost in getting each successive reading. The following 12 were all that Mr MacCarthy and I could obtain, though we worked well all the time we were there, unless for 6 or 7 hours when we went to rest in the boer or farm-house near by, leaving a watch, with orders that we should be called if any unmistakable signs of an approaching eruption were given. From this division of our time it resulted that of these 12 readings, one-half were taken on the evening of the 6th August, and the rest on the morning of the 7th. Allowance must be made for this, in accordance with one of Bunsen's results as to the general rise of temperature in the whole column as a great eruption is coming on.

Temperature-measurements at the Great Geysir, 6th and 7th August 1874.

Depth in feet from surface.	Observed temp. Fahrenheit.	Calculated boiling-point.	Diffs.
0	187°	210°	23°
10½	190°	224°·3	34°·3
18	197°	233°	36
27	211°	241°·8	30°·8

Depth in feet from surface.	Observed temp. Fahrenheit.	Calculated boiling-point.	Diffs.
36	243°	250°·9	7°·9
39	247°	252°·2	5°·2
45	250°·5	257°	6°·5
49·5	254°	260°·2	6°·2
54	256°·5	263°·3	6°·8
58·5	254°	266°·5	12°·5
67·5	*259°·5	272°·2	12°·7
77·5	257°	278°	11°

* Mean of two observations $\begin{cases} 258° \\ 261° \end{cases}$

Bunsen's Measurements in 1846.

6th July, 8.20 P.M.		7th July, 2.55 P.M.		7th July, 7.58 P.M.	
Height from bottom, in feet.	Temp. Fahr.	Height from bottom, in feet.	Temp. Fahr.	Height from bottom, in feet.	Temp. Fahr.
0	254·5	0	261·5	0	259·7
15·75	252·8	16·4	253·4	...	...
31·5	235·4	29·5	248·7	32·3	251·2
47·25	186·4	48·36	223·5	48·36	230·0
63	180·7	64·1	185·4	64·1	184·5

For comparison I give above Bunsen's results from the "Annalen" (1847), with the readings reduced to the same measures as my own. His heights are *from the bottom;* no doubt, because for some time after a great eruption the level of the water in the funnel is gradually rising. While I remained, the basin continued nearly full, the level of water in it not changing more than a few inches.

Bunsen draws the following conclusions from these observations:—

(1.) That, omitting small irregularities, the temperatures in the Geysir-column diminish from beneath upwards.

(2.) That the temperature at all points is dependent on the time since last eruption.

(3.) That that temperature nowhere reaches the boiling-point due to the pressure until a few minutes before a great eruption.

(4.) That the temperature about the middle height of the column comes nearest to the boiling-point corresponding to the

pressure, and approaches it more nearly the nearer the moment of a great eruption.

He argues, therefore. that immediately before such an eruption only a small shock will be sufficient to vaporise a large mass of the whole column, and so to displace the whole column above. Now, it is a fact that the column is constantly subject to such shocks, which occur at intervals of a few hours, and are more frequent as a great eruption is approaching. Bunsen accounts for these shocks, which are in fact abortive attempts at an eruption, in this manner. He observes that it is a feature of most of the Icelandic warm springs that, periodically, at certain points, great bubbles of steam get formed, and rising soon condense in the colder strata above. This is well seen in these rocky cavities, 10 or 12 feet deep, which exist in that remarkable region of springs and mud-cauldrons in the immediate vicinity of the Great Geysir. I observed also something of a like phenomenon at the "quhar" by the side of the lake at Laugardalr, where we rested on the way between Thingvalla and the Geysir. At that spring, however, as the depth of water is quite inconsiderable, the effect is more of a continuous bursting of great bubbles of steam on the surface, as no condensation takes place, the water being at a temperature close on the boiling-point. Now Bunsen argues that, if at some point in the in-carrying ducts of the Geysir-column (and the existence of these ducts is proved by the constant overflow of water from the basin), the temperature of the layer of water gets raised above the boiling-point due to the pressure, owing to the great heat of the surrounding rocks, then a sudden generation of steam is the result, and a rise of that steam in the column itself. This great bubble is soon condensed, while at the same time its sudden formation cooled the water at the point in the duct where it was formed. The phenomenon, therefore, possesses a periodic character, and the explanation, it must be admitted, seems to account well for the conical water-hill, as Bunsen aptly terms it, the sudden upheaval of which in the centre of the basin is an invariable accompaniment of these subterranean explosions, often of very great violence, which are heard and felt recurring at intervals under the Geysir cone. The grand display of a great eruption, however, does not occur until the temperatures in the whole column

are, by the influx of heated water, brought so near their respective boiling-points that a slight upheaval at a certain point of the tube is sufficent to carry the superincumbent layer to a point where, from its temperature and the slight diminution of pressure, a further generation of steam (instead of a condensation of that already formed) will be the result.

Bunsen has shown that the mechanical force which this action developes is fully sufficient to account for the marvellous phenomena of a great eruption. Bunsen's observations and my own agree in showing that it is somewhere about the middle of the column that the observed temperatures approach most nearly to those of the boiling-point due to the pressure. An upheaval of the layer at that point, through only a few feet, will be sufficient to generate instantaneously an additional volume of steam, the pressure of which will again further relieve that of the strata beneath, and so cause an additional volume to be generated there. The enormous force which the phenomenon of the sudden upheaval of a small column of water is thus capable of calling forth cannot be spent in a single eruptive-shot, and hence the explanation of the fact that a great eruption lasts sometimes for four minutes.

No theory of internal steam-cauldrons, filled in succession with steam and water, seems at all consistent with observed phenomena. It fails to explain how, in the abortive eruptions, no water seems to flow from the tube more than the small rivulet which steadily finds its way at the indentations over the rim of the basin. What flows over the margin, and it was great enough, considering its temperature, to cause considerable difficulty in retaining hold of the cord and rope, was due solely to the great commotion in the basin, and was apparently equal to the fall of a few inches in the level of water in the pool when the sudden upheaval had subsided. Further, Bunsen actually let his thermometer remain without injury at the bottom of the tube during a great eruption, having noted on it, a few moments before, a temperature of 9° C. below that of the boiling-point due to the pressure. The column erupted on that occasion he estimated at 43·3 metres or about 142 feet.

It will be observed, that there is a remarkably sudden rise of temperature at a particular point of the column. Thus while the rise between depths of 10½ feet and 18 feet is only 7° F., and that

between 28 feet and 27 feet, 14° F., the rise between 27 feet and 36 feet is as much as 32°. I regret that I did not observe this so as to interpolate one or two additional observations between these. But Bunsen's results (*vide* 3d col.) give one intermediate measurement, his others being in remarkable accordance with mine.

We had several displays of the power of Strokkur, a smaller Geysir about a 100 yards from the Great Geysir. Unlike the latter, it can be made to erupt by throwing in turf, stones, or earth, which stop up the funnel at a point about 27 feet down, where it narrows from a width at the mouth of about 8 feet to about 8 inches. The people living at the farm-house asserted that the long interval of inaction of the Great Geysir was owing to the very frequent eruptions of Stokkur, which had been provoked by way of display during the king's visit.

Professor Tait has suggested the use of a thermo-electric junction, after Becquerel's method, to determine with perfect accuracy the temperature at any point of the column. I believe, however, that no care in packing would make it possible to transport safely a galvanometer and a thermo-electric arrangement over 80 miles of such country as one has to travel to the Great Geysir. I had the misery of seeing the package containing my thermometer repeatedly tossed from the pack-saddle, without any injury to the instrument, however, as I found by comparing it carefully on my return with one tested at Kew. The packages are fastened usually with hair ropes, and not only are these constantly getting loose, but, when a narrow part of the way is reached, the ponies, urged on behind by their drivers, charge against each other, and often leave their loads behind ere they get through.

Setting out from the Geysir at 1 P.M. on Friday (7th August), I reached Reykjavik at 6 the following evening, and, I confess, was grievously disappointed to find that our steamer was not to sail for other 48 hours, an interval which would have sufficed to complete my observations, and would, most probably, have given me an opportunity of witnessing an eruption of this world-renowed spring.

2. On the Capillary Surface of Revolution. By Sir William Thomson and Mr John Perry.

3. **On the Oscillation of a System of Bodies with Rotating Portions. Part II.—Vibrations of a Stretched String of Gyrostats (Dynamics of Faraday's Magneto-optic Discovery), with Experimental Illustrations. By Sir William Thomson.**

4. On the Theory of the Spinning-Top, with Experimental Illustrations. By Sir William Thomson.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VIII. 1874–75. No. 92.

NINETY-SECOND SESSION.

Monday, 3d May 1875.

DAVID MILNE HOME, Esq., Vice-President, in the Chair.

The Portrait of Sir ROBERT CHRISTISON, Bart., Honorary Vice-President of the Society, executed by George Reid, Esq., Aberdeen, was uncovered.

The following Communications were read:—

1. Laboratory Note.—Analysis of Titaniferous Iron Sand from North Berwick. By James Davidson, Esq. Communicated by Professor Crum Brown.

In an excursion of Professor Geikie's class to North Berwick (5th February), a layer of titaniferous iron sand was found lying along the shore for a short distance.

The rocks at this locality consist chiefly of dull green and red trap-tuffs, traversed by dykes and veins of basalt. Immediately in front of the spot where the iron sand was met with several intrusive masses of dark heavy basalt occur. It was evidently from the decomposition and trituration of this rock that the iron sand came, which has been assorted by the waves along the upper margin of the beach.

It was analysed with the following result:—

Magnesia	0·2
Ferrous oxide	17·6
Titanic acid	6·2
Ferric oxide	75·6
	99·6

Specific gravity 4·6.

The sand was highly magnetic.

Assuming that the magnesia is present as titanate of magnesia, and that the titanic acid, in excess of what is required to form titanate of magnesia, is united with ferrous oxide to form ferrous titanate, the residue of the ferrous oxide being united with ferric oxide, we obtain the following proximate composition:—

MgO, TiO_2	0·6
FeO, TiO_2	10·6
FeO, Fe_2O_3	42·9
Fe_2O_3	45·5
	99·6

The sand is obviously crystallised, but the grains are so rounded by rolling as to render it impossible to determine whether they are octahedral or rhombohedral.

If we calculate the quantity of oxygen contained in the several oxides present, we find,—

		Oxygen.
MgO	0·2	0·08
FeO	17·6	4·14
TiO_2	6·2	1·21
Fe_2O_3	75·6	22·68

Or arranged as above.

		Oxygen.
MgO, TiO_2	0·6	0·24
FeO, TiO_2	10·6	3·42
FeO, Fe_2O_3	42·9	12·00
Fe_2O_3	45·5	13·68

Dividing into oxides having the general formula M_2O_3 and M_3O_4 respectively (Fe_2O_3, $MgTiO_3$, $FeTiO_3$; and Fe_3O_4) we have,—

Oxygen in M_2O_3	. . .	17·34
" " M_3O_4	. . .	12·00

These numbers are nearly in the proportion of 3:2, and the composition of this sand might be nearly represented by the formula, $2\,M_2O_3$, M_3O_4.

The composition, however, nearly agrees with that of sands held by Rammelsberg, with good reason, to be mixtures.

2. On some Permian Fishes, hitherto erroneously referred to the Genus *Palæoniscus*. By Dr Traquair.

3. Note on the Action of Bile Salts on the Animal Economy. By J. Graham Brown, Esq. Communicated by Dr M'Kendrick.

The investigations recorded in this paper were undertaken for the purpose of elucidating the action of the bile salts on the animal economy. The chief workers in this field of inquiry have been Frerichs and Kühne, and the results obtained have been to a great extent contradictory.

The most important conclusions at which Frerichs arrives, are:—(1.) That injections of bile into the blood of the lower animals are followed by no important derangement of the vital functions; (2.) That bile pigment is excreted in considerable quantity by the urine after injections of decolorised bile; (3.) That unchanged bile acids are never found in the urine after such injections.—("Clinical Treatise on Diseases of Liver," vol. i. p. 394.)

Kühne, on the other hand, affirms with an equal degree of positiveness:—(1.) That biliary acids are not decomposed in the blood. In whatever manner they find their way into that fluid they are afterwards excreted unchanged by the kidneys; (2.) After injections into the veins of colourless solutions of bile salts, bile pigment may appear in the urine (as stated by Frerichs), but it is due to the property possessed by the bile acids of dissolving the

blood corpuscles, and thus setting free a quantity of hæmatine, which being acted on by the bile acid is converted into bile pigment.—(*Vide* Beale's "Archives of Medicine," vol. i p. 342.)

Neither of these observers notices any abnormal *post mortem* appearances in any of the animals they operated on.

In the prosecution of this investigation there were various courses open by which bile might be introduced into the system. It was not desirable to administer it by the mouth, for then its absorption into the portal circulation, and consequent passage to the liver, would tend to complicate its action. It seemed therefore best to introduce the bile directly into the general circulation by hypo-dermic injection. Intravenous injection was not employed, in order to avoid the error of some who, injecting biliary fluids into the veins, attributed the immediate effects produced on the system to the bile, forgetting that the intravenous injection of any solution, or even of distilled water, will produce such symptoms.

Having then decided to inject the bile into the subcutaneous cellular tissue, it remained to consider what preparation was best for this purpose. Frerichs used filtered bile, but this, though the simplest way, was open to many objections.

It seemed more advisable first to separate from fresh bile the salts of the biliary acids, and to inject these in solution in water. The method of preparation of these salts corresponded very closely with that described by Dr Lauder Brunton in the "Handbook for the Physiological Laboratory." In this manner a mixture of glycocholate and taurocholate of soda is obtained, consisting of nearly equal quantities of these salts. This mixture was dissolved in distilled water so as to make a solution of known strength. In these experiments the solution used contained gr. j. in ♏iij.

The rabbits experimented on were placed in a cage, the floor of which was constructed of glass rods, lying parallel to each other, and very close, and which allowed the urine to pass through into a porcelain dish placed below for its reception, while it retained in the cage the solid excreta. The urine was thus obtained in a very pure state, as it passed through the floor of the cage immediately on being voided. Its quantity was measured in cubic centimeters, as this rendered future calculation much more expeditious.

The urine was tested in the usual way for albumen, bile acids,

bile pigment, and blood. The estimation of the *urea* was attempted by two methods (the one to check the other): *Firstly*, by Liebig's process with nitrate of mercury; and *secondly*, in an easier manner by the use of hypobromite of soda (as recommended by Dr Graham Steele, in the "Edinburgh Medical Journal" for August 1874). These two methods invariably gave results that corresponded very closely, and thus led to greater certainty. But, unfortunately, in the case of both of these processes, other urinary constituents come in to complicate the results of analysis. For not only does *Urea* precipitate the mercury solution, but uric acid, hippuric acid, creatin, creatinine, and even perhaps fibrin, cause precipitation. The same is true with respect to the process with the hypobromite of soda, as these bodies all unite with urea in giving off nitrogen. It is evident, then, that it is impossible by either of these methods to obtain an *absolute* knowledge of the quantity of urea, especially in the urine of herbivora, which contains so much hippuric acid. It was only possible to determine the quantity of nitrogen obtainable from the urine by means of the second process, and to use the other merely as a check. The diet of the animals experimented on was always uniform, consisting of cabbage.

And now, in describing these experiments, it will be best to speak first of those symptoms noticed during life, and second, of the *post mortem* appearances. The former will be given under the system to which each belongs, so that they may be arranged methodically.

Symptoms during Life.

Alimentary System.—No salivation was ever noticed. The appetite was much affected, sometimes almost lost if the dose was large (grains xl.), but with smaller doses (grains x.) it was not much influenced. There was never any vomiting. The bowels were not affected if the dose was small, but generally there was profuse diarrhœa. In all cases the fæces were well bile-stained.

Circulatory System.—The cardiac pulsations were, in most cases, slightly but quite perceptibly decreased in rapidity, a short time after injection. The respirations were similarly affected.

Lymphatic System.—In one case the blood was carefully examined, the greatest precautions being taken to prevent contact with the air as far as possible. In this case from day to day the

coloured corpuscles were seen to have their form altered, while the white increased relatively in number. At the end sometimes more than 40 white corpuscles were to be seen at once in the field of the microscope.

Integumentary System.—No jaundiced line was ever noticed in the skin or conjunctivæ.

Urinary System.—The quantity of the urine was sometimes greatly increased after injection of bile salts, and at other times diminished. This difference depended, apparently, on the amount of food taken during the day following the injection. For when the sole food of an animal consists of a vegetable containing so high a percentage of water as cabbage does, the quantity of urine varies very much according to the appetite; when, however, the quantity of food remained the same, the bile acids seemed to have an undoubted diuretic effect.

The *colour* of the urine in health was almost invariably light. It was opaque owing to a deposit of phosphates. After injection if the quantity was increased, then it became still lighter in colour, but when it was diminished, it became very dark and smoky. This colour was no doubt owing partly to concentration, but its smoky colour led to the belief that some blood pigment was present. This was rendered all the more probable by the fact that blood corpuscles were once or twice seen in the urine after injection of bile salts. However, the spectrum of the urine did not show the characteristic absorption bands of blood on the two occasions on which it was in my power to apply this test.

There once appeared a trace of albumen after injection of bile salts. Small quantities of bile acids could usually be detected after injection, but only rarely was there any trace of bile pigment; it must be confessed, however, that the test for the latter is much less satisfactory than that for the former. The quantity of *free nitrogen** almost always rose after injection of bile salts, as seen in the following tables:—

* By free nitrogen is here meant the nitrogen which is liberated by the action of hypobromite of soda.

First Injection. (Grains x. of bile salts.) October 6, 1874.

	Total Nitrogen obtained.	
Average of 5 days (previous to injection),	510·6	cubic centimeters.
First 24 hours after injection, . .	679·95	„
Second „ „ . .	436·0	„

Second Injection. (Grains xxv.) October 9, 1874.

	Total Nitrogen obtained.	
Average of 2 days (previous to injection),	452·37	cubic cents.
First 24 hours after injection, . .	676·66	„
Second „ „ . .	723·75	„
Third „ „ . .	592·665	„
Fourth „ „ . .	643·5	„
Fifth „ „ . .	486·6	„

Third Injection. (Grains viii.) November 2, 1874.

	Total Nitrogen obtained.	
Average of 6 days previous to injection,	456·25	cubic cents.
24 hours after injection, . . .	356·25	„ (Death.)

(Perhaps the diminution of nitrogenous excretion in this case may be explained in some extent by the rapid death of the animal.)

Fourth Injection. (Grains viii.) November 16, 1874.

	Total Nitrogen obtained.	
First day, previous to injection, . .	673·0	cubic cents.
Second „ „ . .	467·6	„
Third „ „ . .	474·2	„
Fourth „ „ . .	531·2	„
Fifth „ „ . .	591·6	„
Sixth „ „ . .	300·0	„
First 24 hours following injection, .	449·3	„
Second „ „ . .	440·0	„
Third „ „ . .	288·0	„

In this fourth injection there was some diarrhœa on the 5th day, and this reduced the nitrogenous excretion on the 6th day, and so the rise after the injection is not so well seen.

Fifth Injection. (Grains xxiv.) November 21, 1874.

	Total Nitrogen obtained.
Average of 3 days previous to injection,	333·36 cubic cents.
First 24 hours after injection, . .	666·38 „
Second „ „ . .	763·14 „
Third „ „ . .	945·0 „
Fourth „ „ . .	881·9 „ (Death.)

The weights of the animals used for these injections were as follows:—

First injection, . . .	2 lbs. 2 oz.
Second „ . . .	2 lbs.
Third „ . . .	1 lb. 1¾ oz.
Fourth „ . . .	1 lb. 7 oz.
Fifth „ . . .	1 lb. 4 oz.

In the 6th and 7th injections there was such watery diarrhœa that the urine could not be collected separately, and thus the results of analysis were not to be depended upon. In the other cases the urine was not submitted to nitrogenous analysis.

It might be stated as an objection, that as the bile acids were found in the urine, and as they contained much nitrogen, they gave off this nitrogen when treated with hypobromite of soda solution, and thus increased the nitrogen of the urine. No doubt this is true, but only to a very limited extent; for when the biliary solutions used were separately analysed to ascertain how much nitrogen they gave off, it was seen to be so small, that even were the whole solution injected excreted during the next twenty-four hours, it would be utterly insufficient to account for so great a rise in the quantity of nitrogen as was seen to take place after the 5th injection.

Nervous System.—Each injection of bile salts is usually followed by drowsiness. In cases which are to end fatally this deepens, and the animal at last becomes almost comatose. In this state the animal sleeps nearly the whole day, rouses up if touched, but merely moves a little, and again falls asleep. Convulsions were never noticed, but they might have been overlooked, as the animals never happened to be under observation at the time of death.

The pupil never showed any particular change.

Let us now turn to a consideration of the *Pathological appearances* noticed after death. Out of four fatal cases these appearances were only seen in three, as the other animal died so rapidly as not to permit of their development. The lesion seemed to be confined to the liver and kidney, and was of the following nature:—

The Liver was of natural size, but congested, and throughout its substance were scattered numerous small white patches, which contrasted well with the dark red colour of the rest of the organ. The borders of these patches were seldom or never well defined, but the lesion seemed to affect particular lobules. On removing a small piece from one of those white portions of a fresh liver, and teasing it out, one could easily see with the microscope that the liver cells were altered, swollen, with an irregular outline, and full of minute, highly refractive granules. The nucleus was in some cases obscured, in others more distinct than usual. There could also be seen what were evidently the remains of hepatic cells, a nucleus surrounded by a mass of fine oil globules, with perhaps the trace of a cell-wall on one side. If, on the other hand, a portion of the more healthy tissue be examined, liver cells approaching very closely to the normal can be seen.

After hardening in alcohol, or in chromic acid ($\frac{1}{6}$ %), sections of this liver structure may be made, but the changes described are not nearly so well seen then as in fresh specimens. This change in the hepatic tissue is not merely a fatty infiltration, such as takes place, to a limited extent, normally after each meal, but a true fatty *degeneration*. Had it been mere infiltration the lesion would have been general, and not confined to particular regions of the organ; the oil would have been in larger globules, and not in the fine molecular form seen in these specimens; and the hepatic cells would not have been broken down.

The Kidneys were of natural size, but rather pale and flabby. When a transverse section is made across the tubules, the epithelium lining then is seen to be granular and swollen, sometimes to such an extent as to block up the tubule. In order to distinguish clearly that the obstruction in the tubules is composed of swollen epithelium, it is necessary to prepare the sections and render them transparent by treatment with oil of cloves. When this has been

lone the epithelial cells lose their granular appearance, and their outlines can be more readily traced. In a favourable specimen you may then see clearly the slight spaces between each cell converging towards the centre of the tubule, the interstices taking somewhat the appearance of a leech bite.

In other tubules the same appearance may be seen in a more modified degree—the cells swollen, but not to such an extent as entirely to block up the tubule. In other parts of the section tubules may be seen entirely denuded of their epithelium. In longitudinal sections these appearances cannot be so well seen.

A consideration of these facts leads to the following conclusions:—

I. That a mixture of glycocholate and taurocholate of soda when injected hypodermically, in rabbits, in doses of 40 grains and under, does not cause any immediate disturbance, but is almost always fatal (unless the dose be small) in a period varying from 30 hours to 3 or 4 days.

II. That such injections often cause an increased nitrogenous excretion by the urine.

III. That such injections frequently cause diarrhœa.

IV. That such injections are followed by an excretion of a small amount of bile acid by the urine invariably, and in some cases that bile figment is also so excreted.

V. That such injections are followed by a fatty degeneration of the hepatic secreting cells, and of the renal epithelium.

VI. That such injections cause a destruction of red blood corpuscles, and consequently an apparent increase of the white.

VII. That such injections are frequently followed by drowsiness and somnolence.

It would be out of place to attempt to give here a detailed account of the bearing which those results may have on liver disease. It may, however, be right to indicate in a brief way a few of those points which seem to be of special clinical interest.

And, first, in regard to *phosphorus poisoning*, where the liver, kidneys, and muscular fibre become fatty. No true analogy can be traced between the results of these experiments and this disease,

because in poisoning with phosphorus the lesion is fatty infiltration, and not degeneration (according to Niemeyer). There is then only an apparent, not a real similarity between the two conditions.

Disintegration of the hepatic tissue has been occasionally observed to follow obstruction of the bile duct. Frerichs has recorded such a case, but he does not think that this result can be owing to retention of bile in the system; for in some cases where the bile duct was undoubtedly obstructed, disintegration of the hepatic tissue did not occur. But it should be remembered that the bile, though prevented from reaching the intestine, may yet be excreted by the urine. Hence in cases where the kidneys are acting rightly no poisonous effects need be produced. But if, owing to any cause, the urinary secretion be interfered with (as seems to have been the case in the instance Frerichs refers to), then the bile will accumulate in the system, and produce its toxic effects. In cases of jaundice, Graves states* that he was always uneasy as to the issue when nervous symptoms showed themselves,—symptoms, moreover, which he remarked were often *coincident with a diminished secretion of urine*.

But perhaps the most important bearing of the results of these experiments is upon *acute yellow atrophy of the liver*. In this disease there is a rapid fatty degeneration of the hepatic tissue, and of the epithelium of the kidney. In some cases the fatty change is universal through the whole of the liver substance, but in other cases the altered appearance is only seen in *isolated portions*.

Now it seems clear from these experiments that the mere retention of bile acids in the system will, if long enough continued, produce exactly similar changes in these organs. It may be premature to argue from this that the appearances seen in acute yellow atrophy are invariably produced by biliary retention, but that they *may be* so produced is certainly true, and so far as these experiments go they point to that conclusion.

Assuming this to be correct, it will be for consideration how far the jaundice, seen in this disease, may be the *cause* and not the consequence of the hepatic lesion, and whether the efforts of the physician should not be directed more strenuously than heretofore

* Trousseau's "Clinical Medicine," vol. iv. p. 305.

towards the rapid removal from the system of the accumulated bile salts.

N.B.—While this investigation was being carried out, and after the first specimen of the peculiar fatty degeneration had been obtained, there appeared in Robin's "Journal de l'Anatomie et de la Physiologie" for December 1874 an account of similar experiments by MM. Felz and Ritter, professors in Nancy. These gentlemen remarked the same structural changes in the liver and kidneys as have just been described. They also noticed the diarrhœa which follows the injection, and a slight increase in the nitrogen eliminated by the urine. On some points, however, their results are at variance with those recorded in this paper.

While the priority in this investigation belongs without question to MM. Feltz and Ritter, yet it will be seen from the dates of the above experiments that the most important results were obtained previous to the appearance of their valuable paper.

4. Preliminary Note on the Anatomy of the Pia Mater. By Dr J. Batty Tuke.

5. Note on the Physiological Action of Light. By James Dewar, Esq., and Dr M'Kendrick.

The following Gentlemen were elected Fellows of the Society:—

John Christie, Esq., Cowden, Dollar.
James Thomson, Esq., LL.D., University, Glasgow.
Michael Scott, Memb. Inst. C.E.
William Jack, M.A., Glasgow.
James Bryce, M.A., LL.D.

A ballot having been taken, Dr Alexander Wood was re-admitted a Fellow of the Society.

Monday, 17*th May* 1875.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Expiatory and Substitutionary Sacrifices of the Greeks. By Dr Donaldson.

The author gives the results of his examination of the subject in the following propositions:—

1. That the sacrifices of the Greeks were offered to the gods with the idea that the food and drink would gratify them, and that the other offerings would in some way or other be pleasing to them; that the common people continued to offer up sacrifices with this belief till the end of Paganism; but that as the more cultivated classes came to believe that the gods did not stand in need of food, drink, or of gifts from them, substitutions became more and more general with them.

2. That certain sacrifices were intended to appease the anger or overcome the dislike of the gods, not because any sin had been committed, but because the Greek worshipper was not sure of the disposition of the special god towards him, and believed that the wisest course was to conciliate him.

3. That no expiatory sacrifices were offered up simply to express repentance for sin in general, but they were always occasioned by some offence against some individual god or gods; that in these cases care must be taken to distinguish between the purification and the sacrifice; that in the case of deliberate murder no expiatory sacrifice was permissible, but the murderer or his descendants must suffer death; and in the case of involuntary murder, the sacrifice was of the nature of a payment of damages.

4. That there is no instance of a human sacrifice in Homeric times. That in the classical times the one or two allusions really refer to mythical times, and that there is only one instance of human sacrifice for which there is the shadow of historical evidence; that the evidence for this human sacrifice breaks down completely on close examination, and thus we have the fact that

there is no clear proof that one human sacrifice was ever offered up in Greece during the historical period. We have, on the contrary, abhorrence of such sacrifices frequently expressed. Herodotus denounces human sacrifices as an unholy deed (πρῆγμα οὐκ ὅσιον). Æschylus and Euripides * employ language of utmost detestation against it. The Delphic oracle calls it a foreign practice. Pausanias and Porphyrius deem it barbarous. And Sextus Empiricus, contrasting the different feelings of mankind in regard to the same acts, says of the Greeks,—"But *we* think that the temples are polluted by human blood."† The same Greek detestation of human sacrifices is embodied in the tradition that Heracles gained renown by doing away with human sacrifice in various parts of the world.‡

5. That there is no satisfactory proof that the Greeks at any time or in any place were in the habit of offering up human sacrifices. Certain rites may find an explanation in the supposition that human sacrifices were at an early period offered up; but there is no historical testimony to show that the practice ever existed. And even in the cases where the practice may by some be regarded as the best explanation of the rite, we have not a genuine Greek race. The ceremonial on Mount Lycæus was Pelasgic. And the Agrionia and the sacrifices of the Athamantidæ are connected with the Minyan Orchomenos, the seat of Pelasgic worship. So that we should have in these three cases the traditions of the worship of the race which preceded the Hellenes, if we were to base any conclusion on the unsatisfactory information which we have in regard to them. And there are really no other decided cases of what can be regarded as survivals.

6. That the writers of the third period, influenced by the belief that the ordinary gods of the Greeks were demons of savage propensities, lent a ready ear to any tale of horror connected with their worship, and that it is in these writers that we hear of the human sacrifices of the Greeks; but if we place the evidence for these

* Welcker thinks that human sacrifices were attacked by Sophocles in his Athamas, by Achæus in his Azanes, and possibly by Xenocles in his Lycaon. —*Die Griechischen Tragödien*, vol. iii. p. 965.

† Hyp. iii. 24, p. 209.

‡ Welcker.—*Griechische Götterlehre*, ii. p. 769.

sacrifices fairly in the balance, we shall find it not so strong as that which could be adduced to prove that the early Christians killed infants, drank their blood, and indulged in indiscriminate sexual intercourse. And yet no one now believes these accusations against the Christians.

In fact, the Greeks were strangers to the idea of sin until the introduction of Stoicism, as Sir Alexander Grant has well shown in his Aristotle, and it is likely that the idea was not present to the minds of the earlier Stoics. There is therefore, as it seems to me, no analogy between the sacrifices of the Greeks and the sacrifice of Golgotha. The sacrifice of Christ is, as Dr Crawford has admirably brought out in his "Mysteries of Christianity," p. 230, "exceptional and unique." But in the deeper meaning of sacrifice, the essence of which is self-renunciation, there is a striking parallelism between most of the Greek mythical sacrifices, including also the more or less historical voluntary deaths of Codrus and Leonidas, and the sacrifice of Christ. The oracle decrees that what is noblest, and most beautiful, and most fair must perish. The noblest and the fairest offer themselves up for their country, and present to their country the most beautiful sacrifice that can be offered—a pure human soul. And in like manner the sacrifice of Christ, not indeed devoted, like the Greek sacrifices, to a single land, but offered up for the whole world, is an act of obedience to the will of God, and an infinitely grand exemplification of that self-renunciation which constitutes the essence of all true religion.

2. The Placenta in Ruminants.—a Deciduate Placenta. By Professor Turner.

All zoologists, who have accepted the placental system of classification of the Mammalia, agree in placing the Ruminantia amongst the Indeciduata.

As is well known, the fœtal portion of the placenta in Ruminants consists of a number of distinct cotyledons. Each cotyledon is composed of numerous branched villi, which fit into pits or depressions situated in mound-like elevations of the wall of the uterus, called the maternal cotyledons. It is generally believed, that in the process of parturition in these animals, the fœtal villi are

drawn out of the maternal pits without removing any of the maternal substance along with them, just as the fingers are drawn out of a glove without any portion of the substance of the glove accompanying them, so that the placenta is non-deciduate.

Having been engaged in the study of the structure of the placenta in the undelivered cow and sheep, it seemed to me, from the mode in which the fœtal villi divided into branches, and from the consequent subdivision of the maternal pits into smaller branching compartments, that the interlocking of the fœtal and maternal tissues with each other was so great as to render it difficult, if not impossible, for the fœtal villi to be forcibly expelled from the maternal cotyledons, without carrying away with them some of the uterine tissue. With the object of testing the accuracy of this supposition, I procured, in the spring of the present year, the fœtal membranes, separated in normal parturition, both of the cow and sheep, and submitted the fœtal cotyledons to microscopic examination.

Before describing the microscopic appearances, it may be advisable to say a few words on the general arrangement of the cotyledons, both fœtal and maternal, in these animals.

I shall first describe the arrangements in the sheep from a specimen where the cotyledons were well developed.

In this animal the maternal cotyledons projected as cup-shaped mounds from the uterine wall. They were covered on the outer convex surface by the uterine mucosa, which was prolonged as far as the free inverted edge of the cup. The inner surface of the cotyledon was composed of a soft, spongy material, containing numerous pits, which extended almost vertically, and divided as they passed deeper into its substance, into smaller compartments, which radiated towards the outer wall of the cotyledon, without diverging much from each other. The pits were lined by well-marked cells, most of which were irregular in shape, polygonal, ovoid, or even somewhat caudate, and of considerable size, though some few were like modified columnar cells. They consisted of granular protoplasm, in which one, two, or sometimes three, well-defined ovoid or elliptical nuclei were imbedded, but without a cell-wall. Not unfrequently the outline of the individual cells was very indistinct, and they seemed as if composed of a layer of protoplasm studded with nuclei.

The cells rested on a highly vascular sub-epithelial connective

tissue, which formed the proper wall of the pits. The mucous membrane investing the cotyledon was continuous at the mouth of the cup with the walls of the pits in the spongy tissue, so that the cells lining the pits were in the same morphological plane as the epithelium covering the mucosa. The cotyledons were highly vascular. Some of the arteries in the sub-cotyledonary connective tissue were corkscrew-like; and in the deeper part of the cotyledon itself I have seen tortuous vessels. The greater number of the vessels within the cotyledon passed, however, vertically towards the surface lying in the connective tissue walls of the pits; branching repeatedly, as a rule in a dichotomous manner, prior to forming a compact maternal capillary plexus,—not dilating into maternal blood sinuses.

The mucous membrane of the uterus between the cotyledons contained numerous tortuous, branched tubular glands. Some of these extended almost vertically to the surface, and could be seen in almost their entire length in vertical sections—others ran more obliquely, and owing to their tortuosity, were repeatedly divided in vertical sections. The mouths of the glands could readily be seen with a pocket lens opening on the surface, the orifice being partially surrounded by a minute elevation of the mucosa. In the mucosa around the base of the cotyledons, a ring-like series of gland openings were seen. In the mucosa covering the cotyledons glands were also present, but their orifices were much stretched, as if by the pressure due to the great growth of the subjacent spongy tissue of the cotyledon. The sub-epithelial connective tissue in which the glands lay, was not by any means so vascular as that which formed the walls of the pits within the cotyledons. In some sections through the cotyledons and adjacent mucosa no glands were to be seen in the connective tissue intervening between the cotyledon and muscular wall, but they were collected in considerable numbers around the cotyledon, as if pushed outwards by its rapid growth. In other sections, however, tubular glands were seen in the sub-cotyledonary connective tissue; but they seemed to be the deep ends of the branching glands, the stems of which may have inclined obliquely, so as to open on the surface of the mucous membrane covering the cotyledon. None of these subjacent glands, or those situated on the surface of the cotyledon, were seen to open

into, or in any way to communicate with, the pits within the cotyledon itself.

The fœtal cotyledons consisted of numerous villi, which collectively formed a ball-like mass, occupying the concavity of the maternal cotyledon. Each villus consisted of a main stem, which gave off a tuft or cluster of spatulate branches. The villi entered the maternal pits and branched along with them, so that every compartment was occupied by a branch of the villus; but there was necessarily no great divergence of these branches from the main stem. At their deeper end these spatulate branches gave off slender terminal offshoots. The villi were formed of gelatinous connective tissue, in which very distinct fusiform and stellate corpuscles were arranged in an anastomosing network. At the periphery of the villus was a layer of flattened cells, with small but distinct nuclei arranged so as to form an epithelial-like investment. The umbilical vessels ramified within the villus and formed networks of capillaries. The villi were in close contact with the epithelial cells lining the maternal pits. Owing to the inversion of the free edge of the maternal cotyledon and the radiated arrangement of the pits, with their contained villi, it was impossible to disengage the maternal and fœtal cotyledons from each other without drawing away with the fœtal villi portions of the maternal cotyledon. I invariably found that, in drawing the fœtal villi out of their compartments, flakes of epithelial cells accompanied them, which showed how readily this element of the maternal tissue is shed. During parturition, however, when the parts are relaxed, the disengagement of the two structures is necessarily more easily accomplished.

In the cow the maternal cotyledons differed in form from those in the sheep. They were fungiform or umbrella-shaped, and were connected to the uterine wall by a broad neck, around which the uterine mucosa was prolonged as far as the border of the umbrella. The whole convex surface of the cotyledon was riddled with pits, which passed vertically into its spongy substance, and divided into smaller compartments in the deeper part of the cotyledon. Projecting from the wall of each pit were delicate bands, visible to the naked eye, arranged as a rule in a vertical direction, and in the intervals between these bands the wall was perforated by nume-

rous orifices, easily seen with a pocket lens, which were the mouths of depressions or crypts in the wall of the pit, some lying almost at right angles, others obliquely to the wall of the pit itself. The pits, with their numerous crypts, were lined by epithelial cells, similar in character to those of the sheep, and these cells rested on a highly vascular connective tissue, in which the maternal capillaries formed a compact network. But I should state that in the cow a larger proportion of these cells had preserved the columnar form of the epithelium of the non-gravid uterine mucosa.

The surface of the uterine mucosa between the cotyledons presented the mouths of the tubular, branched, utricular glands, which extended more obliquely to the surface than in the sheep, so that in vertical sections through the membrane they were frequently cut through and divided; segments of each gland were as a rule seen, though sometimes the stem of a gland mounted to the surface to open by an obliquely-directed orifice. Glands were also present in the connective tissue forming the neck of the cotyledon, but none were seen to communicate with the pits.

The fœtal cotyledons were situated on the umbrella-shaped maternal cotyledons, and their numerous villi occupied the pits. The stems of the villi were comparatively large, and studded with multitudes of minute tufts, which, arising obliquely or almost at right angles to the main stem, entered and occupied the crypts. The minute villi forming these tufts were so slender and filiform that each terminal offshoot contained only a single capillary loop. The villi were in contact with the epithelium cells, and in drawing them out of the pits, more especially in drawing the tufts out of the crypts, multitudes of the lining epithelial cells came away with them. From the differences in shape of the maternal cotyledon in the cow and in the sheep, there is not the same difficulty in unlocking the fœtal from the maternal placenta in the former animal as in the latter.

For the purpose of studying the shed placenta of the sheep, I procured the after-birth from the ewe as soon as it was passed, and immersed it in strong spirit. Some fœtal tufts were then examined without any other preparation; but others were immersed in glycerine jelly, so as to bind the several constituents of the tuft together. Thin slices were then removed from the hardened tufts,

whilst from others small portions were taken and teezed out with needles. In the examination, a magnifying power of 320 diameters was employed. Quantities of cells, having the form and appearance of the epithelial cells already described, were seen to be intermingled with the fœtal villi. In some cases small patches of cells were seen lying free in the spaces between the villi, but more frequently the cells were isolated. In a few instances I saw groups of such cells in immediate contact with the terminal villi, as if they, in being drawn out of the compartments in the maternal cotyledon, had pulled an envelope of epithelial cells along with them.

When the cotyledons of the shed placenta of the cow were examined microscopically, quantities of granular debris were to be seen floating in the fluid in which the specimens were placed. Along with these granules were small flakes of protoplasm; rounded or ovoid bodies, with distinct outlines looking like free nuclei; and large cells composed of granular protoplasm, containing one, two, or three nuclei, having the anatomical characters of maternal epithelial cells.

The amount of debris and of decidua cells varies considerably in the different slides which I examined; in some being so abundant as to render the fluid in which the specimen was examined quite turbid, whilst in others only slight traces were to be recognised.

From these observations I am of opinion that, both in the sheep and cow, the cotyledons of the fœtal placenta carry away with them, during the act of parturition, a portion of the maternal structure, so that in these animals, and presumably in other ruminants, the placenta is deciduate. So far as my observations have gone, I have only detected the epithelial element of the uterine mucosa, or the cells of the decidua serotina, intermingled with the fœtal villi; but from the bloody state of the external parts of the ewe, for some hours after the birth of the lamb, I think it not improbable that the disruption of some of the maternal cotyledons has been deeper than a mere epithelial shedding,—that the maternal vessels have, in some places at least, been torn across, so as to give rise to the hæmorrhage.

From the observations which I have made on the structure of the placenta in many of the Mammalia, both in the deciduata as well as in the so-called non-deciduata, I am of opinion that the

shedding or non-shedding of maternal tissue, along with the fœtal, during the act of parturition, is determined by the degree of interlocking of the fœtal and maternal portions of the organ during the formation of the placenta, and not from the presence in the deciduata of a structure or structures which do not exist in the non-deciduata. In both forms the same anatomical elements exist, though, as in the case of the human placenta, the maternal constituents may become so modified in arrangement as greatly to obscure their original characters. The fœtal part of the placenta consists of a chorion more or less perfectly covered with vascular villi: the maternal part consists of a modified uterine mucosa, the surface of which is composed of the modified epithelial cells of the mucous membrane, beneath which is a highly vascular connective tissue, the modified sub-epithelial connective tissue of the mucosa.

In those animals in which the chorion remains almost entirely covered by villi, as in the pig, mare, and cetacean, the villi are short, with simple branches, and the depressions, pits, or crypts in the uterine mucosa for their reception are consequently shallow. During the act of parturition the villi are so readily liberated from the uterine crypts that no maternal tissue is necessarily shed along with them, though even here it is not difficult to see that, should the epithelial serotina from any cause become detached from the sub-epithelial connective tissue, flakes of it might pass off along with the villous chorion. In the zono- and disco-placentary mammals, where the villi are much longer, and, as a rule, much more extensively branched, the constituents of the mucosa, both epithelial and sub-epithelial, are so intermingled with the fœtal villi in the region of the placenta as, as is generally admitted, to be shed along with them. The ruminants, therefore, with their scattered cotyledons, are seen to occupy, as regards deciduation, an intermediate position between the animals with a diffused placenta on the one hand, and the zono- and disco-placentary mammals on the other; for whilst the former are apparently non-deciduate during the act of parturition, and the latter part with both the epithelial and the vascular sub-epithelial constituents of the uterine mucosa in the placental area, the ruminants shed, as a rule at least, only the epithelial lining of the uterine pits into which the fœtal villi are inserted.

I have hitherto spoken of the shedding of maternal tissue along with the fœtal during the act of parturition. But, to prevent misconception, it may be well to state that, as indeed has been pointed out by Owen,* by Ercolani,† and by myself,‡ in a memoir previously submitted to this Society, if not during parturition, at least afterwards, all placental mammals are deciduate; for in the pig, mare, and cetacean, "during the period of involution which follows parturition, it is obvious that great changes, either from actual shedding of portions of its substance, or from degeneration and interstitial absorption, must take place in the constituents of the crypt-layer before it can be restored to its proper non-gravid condition."

In the ruminants also, although the epithelial cells may be the only constituent of the uterine mucosa which is shed during the act of parturition, yet, after that act is accomplished, the thick, vascular, spongy tissue of the maternal cotyledon must disappear before the uterus can assume its normal unimpregnated aspect.

It will be observed that in this communication I have given to the term deciduate a more extended signification than has usually been attached to it by anatomists. It has been customary to consider a placenta as deciduate, only when both the epithelium and the sub-epithelial vascular maternal tissue are parted along with the fœtal villi.§ But it appears to me that even when the epithelial lining of the crypts only is shed, the placenta should be regarded as deciduate, inasmuch as there is a shedding of maternal structure, though, of course, in an inferior degree to one in which the sub-epithelial vascular tissue is also separated.

3. An Essay towards the General Solution of Numerical Equations of all Degrees. By W. H. Fox Talbot, Esq., Hon. F.R.S.E.

* The Anatomy of Vertebrates, vol. iii. p. 727. 1868.

† Sur les Glandes utriculaires de l'uterus, &c. Algiers, 1869.

‡ Trans. Roy. Soc. Edinburgh, 1871.

§ Huxley—Lectures on Comparative Anatomy, p. 10. 1864.

4. Note on the Electrical Conductivity of Saline Solutions. By J. G. MacGregor, M.A., B.Sc. Communicated by Professor Tait.

In the *Sitzungsberichte* of the Munich Academy,* Professor Beetz has recently published a review of a paper by Mr J. A. Ewing and myself on "The Electrical Conductivity of certain Saline Solutions," which was read before the Royal Society of Edinburgh during the session of 1872–73, and is published in their Transactions.† I take the earliest opportunity of discussing the strength of the arguments on which his criticisms are based. Unfortunately, on account of Mr Ewing's being at present in South America, I am unable to communicate with him. I alone am therefore responsible for the contents of the present paper.

Professor Beetz begins by characterising the short reference which we made to his valuable work on the conductivity of solutions of zincic sulphate ‡ as a "complete series of mistakes." "My only precaution," he writes, "against polarisation consisted, then, in the use of amalgamated zinc electrodes! Whoever has read and understood my paper, must know that the plan of my work went beyond the experiments with zincic sulphate solutions, that it rather consisted in finding, by a damping method, the conductivity of electrolytes generally, relatively to that of a single one." We are accused of having underrated his precautions against polarisation and of being ignorant of the scope of his work. There were two things which Professor Beetz set himself to accomplish—(1.) to investigate absolutely the conductivity of zincic sulphate solutions; and (2.) to determine relatively to it that of other electrolytes. The first part he has carried out, and his researches in this field are generally regarded as authoritative. The second part, however, he has not carried out. He made use of two original and very ingenious methods for the purpose, but both failed. Of one he says himself, "*Es gelang aber nicht brauchbare Ausschläge zu bekommen;*" § and of the other, "*Auch diese Methode hat mir noch keine Resultate geliefert.*" || Now, however one may

* Sitzungsberichte der Münchener Akademie, 6. Februar, 1875, pp. 59–70.

† Trans. Roy. Soc. Edin., vol. xxvii. part 1, 1872–73, pp. 51–70.

‡ Poggendorff's Annalen, cxvii. 1862, pp. 1–27.

§ Ibid, cxvii. p. 26.

|| Ibid, p. 27.

admire the ingenuity of the methods which he employed, it is impossible to speak of them as having been productive of positive results, and unwarrantable to describe them as methods which have increased our knowledge of this department of Physics. Hence we did not mention his whole plan, nor refer to all the methods by which he endeavoured to carry it out, but confined our attention to that part of it which he had successfully accomplished. If we are justified in having thus restricted our remarks to really fruitful work, we must also be justified in having given as his only precautions against polarisation, those which he adopted in his absolute measurements of the sulphate of zinc. Two sentences will show what these were:—"If we only knew a combination of liquid and electrodes, in which the electric current produces neither polarisation nor new resistances, it could be treated, so far as the measurement of resistance is concerned, exactly as a solid body."* Such a combination is easily obtainable, for "through du Bois-Reymond's investigations it is known that amalgamated zinc electrodes are not polarised in concentrated solutions of zinc vitriol."† The only necessary precaution is thus found; and, as might be expected, one looks in vain for an account of any others. Of course he made his solutions as pure as possible, excluded air, arranged his tube so that it would always be full of liquid, and kept his electrodes in the same positions; but these were not directed immediately against polarisation. His only precaution, in fact, was the only necessary one, viz., the use of non-polarisable electrodes.

In describing Professor Beetz' work, however, we fell into an error by assuming that pure zinc electrodes are not polarisable in solutions of zinc vitriol, and, therefore, regarding it as remarkable that he should have taken the unnecessary trouble of amalgamation; and the same error is seen, as he points out, in our reference to Paalzow's work, in which we say that he used pure zinc electrodes, while in reality he amalgamated them. This is, I think, the only "*Wissensfehler*" of which we can be convicted. As an error it is, of course, to be regretted. But its comparative triviality is shewn by the fact that it not only has no influence on our own work, but does not even affect our criticism of either Pro-

* Pogg. Ann. cxvii. p. 3.

† Ibid. p. 6.

fessor Beetz or of Paalzow. In the case of the former, we attributed to the electrodes which he used the same property as he did himself, and in the case of the latter we attributed to those which we described him as having used, the properties which belong to the ones which he really did use. Nevertheless, Professor Beetz thinks that we did not understand Paalzow's method.* "The authors," he says, "call this method 'very ingenious.' 'Curiously enough!' for they did not understand it at all. If they only had, how ingenious it would then have appeared to them." He comes to this conclusion because he thinks that, had we had a full knowledge of Paalzow's apparatus, we could not have criticised it as we did. All that is necessary, therefore, for me to prove, is that the items of description which we, for shortness' sake, omitted, do not necessitate a change in our criticism. Paalzow's apparatus, according to our description, consisted of two glasses filled with sulphate of zinc solution and joined by a bent tube containing the solution under investigation, the electrodes dipping into the zinc sulphate. Professor Beetz thinks that we had neither grasped the idea that the tube did not open into the glasses containing the vitriol, but into porous clay vessels which contained the same liquid as the tube; nor understood the meaning of his having made two or more measurements of the same solution in tubes of different lengths. Do, then, these facts destroy our criticism? We said that diffusion of the liquids must be a source of error, and that polarisation at the surface of junction might be. These dangers are not excluded even when the clay vessels are used. Diffusion may not go on so rapidly, but it still goes on; the mixture which must take place constantly changes the conductivity, and (as our experiments † shew) possibly to a great extent. Moreover, there are still surfaces of contact, notwithstanding the intervention of the clay vessels, the only difference being that instead of one large one there are numerous small ones; and there is still, therefore, the danger of polarisation ‡ without the possibility of eliminating its effects by calculation. The clay vessel not only

* Monatsberichte der Berliner Akademie, 30. Juli, 1868, p 486. Pogg. Ann. cxxxvi. 1869, p. 489.

† Trans. Roy. Soc. Edin., vol. xxvii. part 1, 1872–73, p. 67.

‡ Monatsberichte der Berliner Akademie, 17 Juli, 1856, p. 1.

does not obviate the two difficulties already cited; it may also introduce a new one, in the form of what du Bois-Reymond calls internal (*innere*) polarisation.* Nor is the second addition to our description more destructive of the accuracy of our criticism than the first. Professor Beetz thinks that, by measuring the same liquid in tubes of different lengths, one may take the difference of their resistances as the resistance of a column of liquid whose length is the difference of their lengths. But this can only be the case on the supposition that the state of matters at the junction of the liquids is the same for both determinations. Now diffusion does not cease at the end of each measurement and wait until the next begins. Nature is not so convenient. Every moment adds to the mixture of the solutions and changes their resistance. If, moreover, there be polarisation at the surface of contact, or (clay vessels being used) if there be also internal polarisation, it must begin at zero and increase from the first moment of contact up to the time of observation (supposing that to occur before the maximum is reached). In order that this condition may be the same for both measurements, the observations must be made after the same lapse of time from the first moment of contact, an occurrence which is manifestly improbable, and, if it should happen, impossible to know. It is true that a judicious choice of electrolytes may remove one or more, though never all, of these sources of error; but such a possibility cannot be taken into consideration in discussing Paalzow's as a general method; while, at its best, as a special method, it has always the defect arising from the mixture of the liquids. How great or how small the error arising from mixture and polarisation may be, it is difficult to say. That can only be decided by future experiments. But it is clear that the error remains, and that the method, as described most minutely, is subject to the same criticism as in its simpler form.

Passing from Paalzow, Professor Beetz proceeds to prove that we did not understand Kohlrausch and Nippoldt's † work any better

* Monatsberichte der Berliner Akademie, Aug. 4, 1856, p. 15, and Jan. 31, 1859, p. 1.

† Göttinger Nachrichten, Nov. 18, 1868, p. 415. Jahresbericht des phys. Vereins zu Frankf. 1867–68, p. 71. Pogg. Ann. cxxxviii. 1869, pp. 280 and 370.

than that just discussed. We ourselves are, however, to a certain extent responsible for this judgment. The cause of his misunderstanding is the careless structure of our description. By the use of a specific term in the second sentence instead of a generic, the thermo-electric currents are made to appear to have been used as a means of reducing the electromotive force of the magneto-electric currents, while in reality the former were substituted for the latter in order to obtain currents of low as well as of high electromotive force, that is, to reduce the electromotive force of the currents employed. The sentence is a parenthesis, in which the words "these currents" take, from the structure of the preceding part of the description, a narrower meaning than they were intended to have. One might almost have expected Professor Beetz to discover the defect, rather than adopt the supposition of belief in the reduction of the electromotive force of magneto-electric currents by means of a thermo-electric pair.

The casual remark that the resistance of either a wire or a constant cell can be easily measured, Professor Beetz translates incorrectly, and, in consequence, criticises unfairly. We did not affirm that the resistance of a galvanic cell is quite as easy (*ebenso leicht*) to measure as that of a solid body, nor did we mean to say that there is yet a perfect means of measurement, but that by Wheatstone's method (in our reference to which Galvanometer is printed Electrometer) approximately accurate results may be easily obtained. Why then have von Waltenhofen and others tried to improve upon Wheatstone? Simply because they think they can reduce the already greatly diminished sources of error, or because they wish to have a method applicable to both constant and inconstant cells.

I come next to consider Professor Beetz' criticism of the method of resistance measurement which we used; and it is interesting to notice how even he, who, having written what he has written, might be expected to take all possible precautions against mistake, nevertheless can slip into what he would call, if we had been guilty of them, *die allergröbsten Wissens- und Verständnissirrthümer*. He thinks that our method is the same as his own, except that we substituted platinum for zinc electrodes, and relied upon the quickness with which we could make and break contact for the measuring

of resistance during the earliest stages of polarisation! Accordingly, his criticism is, that contact cannot be so quickly made and broken, *i.e.*, the time of passage of current cannot be made so short, as to warrant our neglecting the effect of polarisation; and he cites as proofs of the fact the experience of Kohlrausch and Nippoldt,* and the experiments of Edlund.† "If," he writes, "the authors were acquainted with Edlund's experiments, they would know that platinum electrodes, between which the current from three Daniell's cells has passed in dilute sulphuric acid during only $\frac{1}{50}$th of a second, have already acquired a polarisation, whose electromotive force is equal to that of 0·57 of a Daniell's cell." Very interesting, but unfortunately not to the point! Any one who understood the method of our paper would know that Edlund's experiments were quite irrelevant, simply because our observation was equally accurate whether the current flowed during $\frac{1}{100}$, 1 or 100 seconds. In what respect then did the method differ from Professor Beetz' idea of it? In one respect, viz., that while he supposed us to have used a heavy mirror galvanometer, we used Sir William Thomson's "Dead Beat" galvanometer‡—and we distinctly stated that such an one was necessary for the use of our method.§ This galvanometer has four peculiarities:—(1.) The mirror is exceedingly light; (2.) On the back of it there are four very small magnets;‖ (3.) The mirror cell is just large enough to admit of deflection; (4.) The front and back of the cell act as stops. In virtue of the former two peculiarities the mirror moves almost instantaneously in obedience to even a very weak current; and in virtue of the latter two, there is almost no oscillation. The effect of these properties is seen by comparing an observation made by means of the ordinary galvanometer with one made by means of the "Dead Beat." Suppose the ordinary galvanometer to be used in a Wheatstone's Bridge, one of the arms of which is a tube containing an electrolyte with platinum electrodes, while the other three are known resistances, and are

* Pogg. Ann. cxxxviii. p. 282, 1869.

† Ibid. lxxxv. p. 209, 1852.

‡ See Fleming Jenkins' "Electricity and Magnetism," p. 198.

§ P. 58 of our paper.

‖ The mirror and magnets of our galvanometer weighed together only about ·03 grm.

arranged in such proportion that, if the tube were replaced by an equal metallic resistance, a very small deflection of the mirror, in a positive direction, would be obtained on closing the circuit. Then, during the time of contact, a large deflection is produced in a negative direction. The moment of inertia of the mirror is so great, that before the main current has moved it, at least perceptibly, in the positive direction, the polarisation current carries it off in the negative. If contact lasts $\frac{1}{50}$th of a second, the deflection is due to the sum of all the forces acting upon the mirror during that space of time. Against such a method Professor Beetz' criticism is valid; because it is almost impossible to make and break contact in less than $\frac{1}{50}$th of a second, and we certainly did not think that we had done so. But suppose that we use the "Dead Beat" galvanometer, the bridge being in the same condition. During contact the mirror makes two deflections,—the first, very small and in a positive direction, the second, much larger and in a negative direction,—the size of the second deflection depending, within limits, upon the length of time of contact, while both the occurrence and size of the first are entirely independent of it. The inertia of the mirror is so small, that the main current—lessened of course by the first traces of polarisation—produces its effect before polarisation has had time to gather its forces; and it is this first deflection, caused by the main current, which is observed, and which is reduced to just nothing by changing the relation of the arms, in order to determine the resistance of the tube. It is thus evident that the length of time of contact has no effect upon our result. So far as a single observation is concerned, it is quite the same whether it lasts a long or a short space of time. Edlund's experiments and Kohlrausch and Nippoldt's experience are thus alike worthless to a critic of our method, and Professor Beetz cites them simply because he was criticising a conception of his own. He was, perhaps, led astray by the importance we attached to making the time of contact as short as possible. But this precaution had reference to the next following observation. The shorter the contact, the less time required for depolarisation and the less change in the constitution of the liquid. The same remarks, which I have made to shew that we did not lean upon such a broken reed as the shortness of contact, make it evident also that we were right

in our choice of platinum electrodes. Professor Beetz' own experience,* and that of other workers in this department, shew that platinum electrodes are the best to form part of an apparatus for the investigation of electrolytes generally.

The method which we used certainly cannot be regarded as completely eliminating the effects of electrolytic action. Like the other general methods which have been tried, it is an approximation method. It aims, just as Kohlrausch's does, at measuring resistance when the effects of polarisation are so slight that they may be neglected. If the weight of the mirror were indefinitely small, its first deflection might be regarded as due to the main current alone. But it is impossible to obtain such a mirror; and the first deflection must be regarded as caused by the sum of the forces of the first few moments, which sum includes the electromotive force of polarisation. It is generally admitted, however, that polarisation, beginning at zero, must during the first few moments be exceedingly slight. The mirror, therefore, being sufficiently light, the observation will take place when its electromotive force is so small as to warrant its being neglected. The lighter the mirror, the more rapid the deflection, the smaller the influence of polarisation and the error. Whether or not, with the lightest mirror which can at present be made, the error is really small enough to be neglected, can only be determined by a comparison of results with those of some standard method which eliminates completely the effects of electrolytic action. Our method guards for a single observation not only against polarisation, but in the same way against the effect of the production of substances which, while giving rise to no new electro motive force, differ in conductivity from the original liquid. The observation is made before such products can be formed in any appreciable amount. The necessity of repeated observations in the same liquid, however, renders the elimination of this error only slightly less defective than in the methods of Kohlrausch and Nippoldt, Kohlrausch and Grotrian, Paalzow, and perhaps even Professor Beetz.

In passing to consider the numerical results which we published, Professor Beetz refers to two remarks which we made about his results, in one of which he finds the only *Verständnissfehler*, of

* Pogg. Ann., cxvii. p. 26.

whose occurrence in our paper I am aware. We said that in his researches on the conductivity of solutions of different density, he was not careful to keep to exactly the same temperature throughout a whole series of observations, so that his results did not admit of accurate graphic representation. This is a mistake, proceeding probably from a misunderstanding of one of his tables, and I am happy to have this opportunity of making the correction. In speaking, however, of our having made observations at constant temperatures, Professor Beetz' statements are somewhat too sweeping. He proves it himself; for the confidence which he places in Paalzow's* numbers shew that this is not the cause of his want of confidence in ours. Our experience is contrary to his opinion. We found the method of constant temperatures somewhat slow, but met with no great difficulty in making our measurements always within a small fraction of 10° C. Had it not been so, it would have been easy to adopt the method of interpolation.

The second remark was merely a statement of fact, and not intended, as Professor Beetz thinks, as a reproach. Which of the two kinds of formula is the better, is, of course, simply a matter of opinion. We wished merely to state that Professor Beetz, having adopted the usual one, had not availed himself of what we regarded as one great advantage of the other. He thinks, however, that we not only made the worse choice, but did not produce good specimens of the kind which we had chosen. "It would be better," he says, "if there was more agreement between their observed and calculated numbers." As a comment upon this remark, I give the tables of conductivity (observed and calculated) of the solutions which both he and we have examined. His results we take from the table given in his paper,† the latter part of which has been omitted, because our formula (a manifest disadvantage) does not apply to solutions beyond that of maximum conductivity. Our numbers are those given in our agreement-table,‡ reduced to the same form as his, the first four being omitted because they correspond to solutions weaker than any that Professor Beetz examined. All the given numbers must be multiplied by 10^{-9}.

* Monatsberichte der Berliner Akademie, 30. Juli, 1868, p. 488.

† Pogg. Ann. cxvii. 1862, p. 20.

‡ Trans. Roy. Soc. Edin. xxvii. part i. 1872–73, p. 64.

Beetz' Table.			Ewing and MacGregor's Table.		
Observed.	Calculated.	Difference.	Observed.	Calculated.	Difference.
2387	2315	+72	1876	1883	− 7
2864	2864	0	2264	2264	0
3417	3408	+ 9	2828	2828	0
3921	3992	−71	2969	2997	−28
4450	4487	−37	3145	3166	−21
4502	4502	0	3264	3298	−34
4528	4545	−17	3344	3344	0
4594	4615	−21	3367	3379	−12
4638	4621	+17			
4641	4630	+11			
4626	4638	−12			
4628	4641	−13			
4632	4649	−17			
4640	4651	−11			
4632	4645	−13			

A comparison of these tables shews that, within the limits of common observation, Professor Beetz' remark will apply as well to the agreement of his own numbers as of ours. That the agreement for very dilute solutions is not so good we have already satisfactorily explained.* Professor Beetz did not attempt to measure their conductivity, because he found that when there was less than a certain percentage of salt in his solutions, his method was no longer proof against the effects of polarisation.

I have already said that the only way of testing our method is the comparison of the results which it furnishes with those of a known perfect method. But the comparison to which I refer is not such an one as Professor Beetz has proposed and executed. Its first condition must be the possession of a standard method; its second the elimination of all unnecessary variables. It must be such as to allow suspicion of the cause of differences in results to rest only upon the method itself. If there be x sources of error, it cannot be fastened upon any one. The solutions examined must be the same, the vessels containing them the same, the standards of resistance the same. The only pieces of apparatus which *may*, are those which *must* be changed. The fulfilment of the second condition is easy. The first is generally regarded as fulfilled by the use of Professor Beetz' method. Kohlrausch and

* Page 64 of our paper.

Grotrian used it in testing the validity of their method.* That, however, Professor Beetz' method is free from all the disturbing effects of electrolytic action seems not yet to be thoroughly established. Du Bois-Reymond's experiments show that there is no polarisation. But do Professor Beetz' experiments † prove that new compounds are not formed by the passage of the current, of different conductivity from that of the original liquid? In the tube which contained the electrolyte he placed several pieces of amalgamated zinc, which fitted the tube closely like pistons. Holes were bored through the axis to enable liquid to pass from one side to the other. Generally, he says, the pieces of zinc, so soon as they touched, clung fast to one another (*hafteten fest an einander*), but they could always be separated by inclination of the tube. When all the pieces were lying close together, and one of them close to a zinc electrode, the current would have to pass only once through the electrolyte; but when they were separated from one another, it must several times pass through the solution. If any new resistance were produced there would be several times as much produced in the second as in the first case, and the fact could be observed. This would be conclusive, if it was certain that there was contact between the pieces of zinc lying next one another. If the amalgamated surfaces had been coated with liquid amalgam, there would be sufficient certainty of contact to warrant trust in the experiment. But that does not seem to have been the case, as Professor Beetz does not mention it, and therefore leaves his readers free to suppose that he did not rely upon its agency. He must then have taken for granted that, without the use of any pressure, the pieces of zinc could be brought so close together as to prevent the liquid from penetrating between them, —a somewhat doubtful supposition. If there was a layer of electrolyte, however thin, between each two neighbouring pieces of zinc, electrolysis would occur at as many points of the tube as when the zinc pieces were farther separated, and the resistances observed would be equal whether compounds of greater or less resistance were formed or not. Hence, until more is known of the mode in which Professor Beetz assured himself of the contact

* Pogg. Ann. cliv. p. 9.

† Ibid. cxvii. pp. 6–8.

of his pistons of zinc, his results must be looked upon as questionable.

While in Professor Beetz' comparison his fulfilment of the first condition is not without doubt, his fulfilment of the second is certainly not faultless. He forgets the certainty of error arising from the use of different standards of measurement, as well as the certainty of still greater error from the use of different vessels for holding the electrolyte. He recognises, however, the necessity of knowing that we worked with the same substances, and this fact he proves in a somewhat extraordinary way. His argument takes the form of a hypothetical syllogism:—If we used the same substances, both methods must have indicated the same solution as that of maximum conductivity. Now both methods have done so (this itself was only approximately the case). *Ergo*, we used the same substances. The fallacy is evident. He has mixed up what the logicians call the *modus ponens* with the *modus tollens*, and forgotten that a conclusion can be drawn only from a denied, never from an affirmed "consequent." That Professor Beetz intended the "major premise" as we have given it, is evident, from the fact that, while he could reasonably expect its admission from his readers, he could not expect them to grant the converse, viz., that if both he and we had indicated the same solution as that of maximum conductivity, we must have used the same substances. Such an assumption would make his syllogism correct. But it neglects what the syllogism itself, according to the first version, neglects, viz., the fact of the possible plurality of causes.

While the method which we used cannot, by Professor Beetz' argument, be proved faulty, the results which we published might thus be shown to be unreliable. For that purpose it would be necessary that various methods of acknowledged approximate accuracy should give results approximately the same, and differing widely from ours. Professor Beetz thinks this has been done. "Kohlrausch and Nippoldt have shown how close is the agreement between the results which they, Paalzow, and I have obtained in three quite different ways. The agreement between their measurements of zinc-vitriol solutions and mine, and between their measurements of dilute sulphuric acid and Paalzow's, is perfectly satisfactory." One would understand from these sentences that

each of the three experimenters mentioned had used in all his (or their) observations a method quite different from that of the others; and that, while the results of one pair on zinc-vitriol agree, the results of the other pair on dilute sulphuric acid also agree. Kohlrausch and Nippoldt's determinations are used as a medium of connection between those of the other two; and only on the supposition that they used throughout the same method or methods connected by compared results, can they reasonably be used as such, or the agreement between Kohlrausch and Nippoldt and Paalzow be supposed to assist in establishing the accuracy of Professor Beetz' results. What, then, are the facts? Professor Beetz has made a series of observations of the conductivity of zinc-vitriol solutions; Paalzow, of dilute sulphuric acid. Kohlrausch and Nippoldt have investigated solutions of both, *but with different and unconnected methods.* In all cases in which they investigated zinc sulphate they used Professor Beetz' method. The principle of his method is the use of non-polarisable electrodes; that of Kohlrausch and Nippoldt's, the reduction of polarisation by means of rapidly alternating currents and large electrodes to as small an amount as possible. In one determination their mode of investigation was quite the same as Professor Beetz'; * in the other they used magneto-electric alternating currents instead of the ordinary galvanic current.† It is evident, however, that, even in this determination, since they used amalgamated zinc electrodes and not platinum ones, they were working with Professor Beetz' method and not their own; for the alternating currents are characteristic of their method, only in so far as they prevent the heaping-up of the polarising substances on platinum or other polarisable electrodes. A link is wanting, then, between the methods used by Kohlrausch and Nippoldt in their sulphuric acid determinations and their zinc sulphate determinations respectively. Nor is this link supplied by the comparison of methods given by Kohlrausch and Grotrian; ‡ for they make only a single comparative observation, and their platinum electrode method is an improvement upon that of Kohlrausch and Nippoldt.§ There being no connecting link, Professor Beetz cannot cite

* Pogg. Ann. cxxxviii. p. 376.

† Ibid. p. 378.

‡ Ibid. cliv. p. 10.

§ Ibid. p. 2.

Paalzow's agreement with Kohlrausch as evidence for his own accuracy, and his "agreement of results obtained by three observers in three quite different ways" becomes *the agreement of results obtained by two observers in the same way.* We must now inquire what even this agreement amounts to. Kohlrausch and Nippoldt made two comparable observations. The first, however,* is rendered worthless by the fact that they assume the resistance of two vessels of the electrolyte, apparently without having made any accurate determination. The second† agrees very well with Professor Beetz' corresponding determination; but in order to conclude from this single agreement to general agreement, the unwarrantable assumption must be made that the error found is not less than the average error.‡ Kohlrausch and Grotrian, whom Professor Beetz also cites, make two comparable observations, but both are questionable from the fact of their being unable to state accurately the constitution of the solutions whose resistance they measured. The authority with which Professor Beetz condemns our results as inaccurate on the ground of non-agreement with the agreeing results of various quite different methods may now be judged by the reader for himself.

That the results of a new method applied by young experimenters should be even approximately accurate is, perhaps, hardly to be expected, and it will probably be found that our numbers are not quite exact. If Professor Beetz' conclusion were well grounded they would need to be corrected only to the extent to which

* Pogg. Ann. cxxxviii. p. 373. † Ibid. p. 376.

‡ The "perfectly satisfactory agreement" between Kohlrausch and Nippoldt and Paalzow is based also upon comparison of a single pair of observations, the same unwarrantable supposition being made. With regard to this agreement it is interesting to notice the fact that Kohlrausch has lately corrected his first published numbers to the extent of 4 per cent. Paalzow's observed conductivity instead of being a little more than 2 per cent. less than Kohlrausch's, becomes a little less than 2 per cent. greater. If Paalzow were next to make the same discovery there would still be the same agreement, and Professor Beetz' argument would be untouched. Even if such corrections should proceed alternately, *ad infinitum*, his argument would hold at all stages of the process as well as at the present! It would still be true that Professor Beetz agreed with Kohlrausch and Kohlrausch with Paalzow, and therefore Professor Beetz would be proved to be authoritative. So long as Kohlrausch and Paalzow agree,—it matters not whether in accuracy or in error,—they nevertheless prove Professor Beetz accurate!

Kohlrausch has already corrected his first results to be made "perfectly satisfactory." We might therefore congratulate ourselves on having made so close an approximation, and proceed to lighten our mirror. Whether or not, however, it must be much or little or at all lightened, is a question which must be regarded as not yet settled. It may be that Professor Beetz' results are accurate, and that ours alone need correction, but that is not proved; and, in the meantime, it will be well to hold to the acknowledged truth that neither accuracy nor error is often found to be all on one side.

Professor Beetz "would not have pointed out the weak points of our paper in so searching a style, had we not conducted the experiments under the guidance of Professor Tait," who, in communicating it for us to the Royal Society, took upon himself, he thinks, the responsibility for its contents. He did so, at the very least, to the same extent as is always done by the secretary of a learned Society who communicates a paper not written by a Fellow. The remarks which I have offered will, I venture to think, shew the responsibility to be a lighter matter than Professor Beetz supposes. If our errors were as numerous as his accusations there would certainly be a great deal to answer for. But fortunately the strength of the arguments on which he bases them is inversely as the strength of language in which he expresses them; and as the latter is great, so the former is small.

Monday, 7th June 1875.

DAVID STEVENSON, Esq., Vice-President, in the Chair.

The following communications were read:—

1. On High Flood Marks on the Banks of the River Tweed and some of its tributaries, and on Drift Deposits in Tweed Valley. By David Milne Home, LL.D.

In many parts of Scotland there are indications that our existing rivers reached much higher levels than at present, and that

large bodies of water prevailed over districts which are now dry land.

As these facts suggest important speculations as to the physical conditions and climate of the country, it is desirable that they be investigated, before becoming more indistinct from the combined effects of weather and land improvements.

The author divided his paper into the following heads:—

1. Water lines on the banks of the River Tweed and some of its tributaries.
2. Notice of drift deposits, and appearances of ancient lakes.
3. Theoretical explanations suggested.
4. Notice of views by other persons.

I.—*Water Lines on Banks of the Tweed.*

(1.) The lowest, and therefore the most recent, water line is indicated by haugh-lands bounded by a cliff or bank, and at a height above the present ordinary summer level of the river of from 14 to 22 feet, at various places (which were specified) between Berwick and Melrose. The line is low where the floods have room to expand laterally; high, where the banks are near one another and vertical. To the above-mentioned heights the river rose above its present channel on 9th February 1831, being the greatest flood which has occurred during the last hundred years.

(2.) There are two higher water lines, from 22 to 50 feet above the present channel of the river. These being older, they are less continuous and less distinctly marked. With regard to them the question is, were they reached by the river from its present channel, or when its channel was higher?

(3.) Another extensive flat, more or less horizontal, but apparently not produced by the river, is at a height of from 115 to 130 feet above the sea. It is seen on both sides of the Tweed, but not beyond Kelso.

(4.) Traces of two higher flats or terraces exist in the districts adjoining the Tweed Valley—one from 170 to 180 feet, the other from 200 to 220 feet above the sea.

II.—*Notice of Ancient Lakes and Drift Deposits.*

(1.) There are indications that lakes existed formerly in Lauder Valley, at Huntly, near Grant's House, on the north side of Tweed valley; and at Morebattle and Millfield Plain, on the south side of the valley, as well as at many other places.

(2.) The drift deposits consist of clay, gravel, sand, and boulders. The clay generally occupies the lowest parts. Extensive beds of sand exist from the lowest parts up to 1000 feet above the sea and more. Gravel lies more frequently over sand than below it. These deposits form round hills, as also extensive elliptic shaped ridges. These ridges are generally parallel to one another and to the axis of the valley. To the west and north of Kelso their average direction is (by compass) E.N.E; near the sea, about E. and W. There are also, at a level of about 800 feet above the sea, remarkable eskars or kaims, running continuously for more than a mile, and observing approximately a parallelism with hills not far distant. Boulders are of three classes—some from parent rocks situated in the valley, some from rocks in the neighbouring hills, some from rocks in the Highlands.

The localities where striated rocks occur were pointed out.

III.—*Theoretical Explanations.*

The author, to account for the beds of stratified sand and gravel, and for their formation into parallel ridges, as well as for the transportation of boulders, assumes that sea, loaded with ice, prevailed over the district to a height of 1500 feet and more. He infers also that, after the sea began to sink towards its present level, a kyle or arm of open sea prevailed between the Cheviot hills on the south and the Lammermuir range of hills on the north, the shallowest part of which would be the present watershed between the counties of Roxburgh and Dumfries—viz., at St Mary's Loch and Mosspaul, which are about 800 feet above the present sea-level. During the period that the level of this sea continued to sink towards the present level, pauses probably occurred in the process, which would allow of the formation of cliffs by the undermining or erosion of the land, and also the formation of flats or terraces by the deposit of sediment.

A particular account was given of various localities where flat land or terraces in Tweed valley, at various heights above the sea, were recognisable. When the flat of 115 to 120 feet existed, the sea reached to Kelso; and there, at that time, the Tweed would join the sea. As the sea fell—say 30 or 40 feet, to Coldstream—the River Tweed would cut out a deeper channel for itself, and when the sea fell so much more, its channel would be still more deepened, till it reached the present sea-level. During these periods, when the river ran in one channel after another, flood-marks would be made on its banks, traces of which would long remain, though it is only the most recent which can be expected to be now visible.

IV.—*Views of other Persons.*

1. As to the drift deposits—

(1.) Several geologists have ascribed the formation of the parallel ridges of drift deposits in Tweed Valley to the action of land-ice, and suppose that some of these ridges are *lateral*, others *terminal* moraines.

(2.) They have also been ascribed to *fluviatile* action, aiding that theory by the supposition that enormous floods were in former times caused by the climate being more rainy, or by the melting of ice and snow on the hills.

The author combated both views, holding that as similar ridges of sand, gravel, and mud are formed now in the sea, so they may have been formed in this district, when the district was under the sea.

2. As to the high flood-marks on the river, reference was made to the opinions of Mr Alfred Tylor, and of the Rev. Thomas Brown, of this society, as to the probability that those marks were made by the rivers flowing in their existing channels.

The author combated this theory, stating, that when the sea stood at higher levels all the rivers of the country must have likewise flowed in channels at higher levels, and that the flood-marks in question were formed then.

2. Observations on Mr Sang's Remarks relative to the Great Logarithmic Table compiled at the Bureau du Cadastre under the direction of M. Prony. By M. F. Lefort. Communicated by Mr Sang, who has translated the paper from the French.

To the President and Council of the Royal Society of Edinburgh, Scotland.

Paris, *le* 29 *Mars* 1875.

Monsieur le President,

J'ai reçu par une voie détournée, un article de M. Edward Sang, intitulé "Remarks on the great Logarithmic and Trigonometrical Tables computed in the Bureau du Cadastre under the direction of M. Prony." Cet article, qui parait avoir été publié dans les "Proceedings of the Royal Society of Edinburgh, Session, 1874–1875," m'a été adressé à l'Observatoire de Paris. Or je n'ai pas l'honneur d'être astronome. Je suis simplement un inspecteur général des Ponts et Chaussées, ami de la science qu'il a cultivée et qu'il cultive encore dans les trop courts instants de loisir que lui laisse sa carrière professionelle. L'Observatoire de Paris a d'ailleurs bien voulu me renvoyer la brochure à mon domicile, rue du-bac No. 38, à Paris.

Quoique j'aie une opinion faite sur le fond de la controverse qui s'est élevée entre la rédacteur de la feuille périodique, intitulée "Nature" et M. Edward Sang, je crois qu'il ne serait ni opportun, ni convenable que j'exprimâsse un avis. Mais, il est des questions de fait et de doctrine soulevées par M. Sang, qui me touchent trop personellement et intéressent trop la science, pour que je ne regarde pas comme une devoir d'éclairer, dans les limites de mon pouvoir, des savants qui se sont occupés de mes travaux avec tant de bienveillance.

Tel est l'objet, Monsieur le President, de la note ci jointe que je vous prie de vouloir bien soumettre à la Société Royale d'Edinburgh, et de porter à la connaissance de M. Edward Sang, dont le domicile m'est inconnu.

Veuillez agréer, Monsieur le President, l'assurance de ma haute considération et de mes sentiments respectueux.

F. Lefort.

Observations relatives aux remarques publiées par M. Edward Sang dans les "Proceedings of the Royal Society of Edinburgh, Session 1874–1875," sur les grandes tables logarithmiques et trigonomètriques calculées au Bureau du Cadastre sous la direction de Prony; par F. Lefort, Inspecteur général des Ponts et Chaussées, membre correspondant de l'Académie des Sciences de Naples.

M. Edward Sang, dans un article dont je viens de rappeler le tître, a mentionné de la manière la plus flatteuse les travaux que j'ai publiées sur la matière des logarithmes, et notamment sur la grande operation qu'a dirigée Prony à la fin du siècle dernier. Je ne puis que l'en remercier; n'entendant d'ailleurs intervenir en aucun façon dans le fond de la controverse qui s'est engagée entre ce savant et le rédacteur de la feuille scientifique intitulée "Nature." Mais je lui dois, autant qu'à la très honorable Société Royale d'Edimbourg, des explications sur différents points de fait et de doctrine, qu'il regarde comme ressortant de mes écrits, écrits qu'il a mal interprètés, sans doute par suite d'une connaissance incomplète de la langue Française. Je n'ignore pas qu'en rédigeant cette note je m'expose à un danger de même nature; mais je compte sur l'indulgence de M. Sang, comme il peut être assuré de la mienne.

I. M. Edward Sang n'admet pas que les tables de Vlacq, corrigées au moyen de mon *errata*, puissent suppléer les tables nouvelles dont il propose l'impression. Il établit d'abord qu'on ne peut se procurer les tables de Vlacq qu'à un prix élevé; qu'il est encore assez difficile d'avoir un exemplaire du 4^e volume des Annales de l'Observatoire de Paris; enfin, qu'il n'existe pas une concordance parfaite entre les divers exemplaires de l'ouvrage de Vlacq. A l'appui de cette dernière thèse il cite la phrase suivante qui serait imprimée à la page 64 des tables de Taylor; "in about 100 copies; in about 200 copies; doubtful whether a few copies are erroneous or not; in about half the impression; only in one copy; and so on."

Je possède une édition des tables de Taylor, publiée en 1792, à Londres, par les soins de Maskelyne. J'y trouve à la page 64 un errata, avec cette mention fort différente de celle qui précède: "Errata of the logarithmic tables, which affect only part of the impression of the sheet, and have been corrected by the printer

since the impression, except any may have escaped correction through inadvertence."

Y-a-t-il eu plusieurs éditions des tables de Taylor? je n'en sais rien. Mais il résulte bien de la préface placée en tête de l'édition que je viens de citer, que la publication a été faite pour la première fois par les soins de Maskelyne. En tout cas, les deux citations me paraissent s'appliquer exclusivement à l'ouvrage de Taylor, et n'avoir aucun trait à l'ouvrage de Vlacq.

L'errata que j'ai donné dans le 4^e volume des annales de l'Observatoire, est relatif à l' "Arithmetica logarithmica . . . . . per Adrianum Vlacq Goudanum, Goudae 1628. Petrus Rammasenius," et non aux contrefaçons qui ont pu se produire.

Quant à l'hypothèse des caractères "moveable types" enlevés par l'ouvrier manœuvrant le tampon à encre "inking dabber," et qui auraient été mal replacées par l'ouvrier qui fait agir la presse "pressman," je ne puis comprendre qu'elle soit produite; car elle suppose l'absence complète des vérifications les plus habituelles, les plus élémentaires, et dont l'omission est d'autant moins probable dans l'espèce que Vlacq, l'auteur de ces tables si précieuses, avait été imprimeur et le prédécesseur immédiat de Petrus Rammasenius.

J'admets bien qu' à la suite des tirages successifs on ait pu rendre plus corrects les exemplaires de Vlacq, mais je n'admets pas qu'on les ait rendus moins corrects. Il est donc possible que l'exemplaire dont on dispose, ne renferme pas toutes les erreurs successivement signalées par Vlacq, Véga, et autres auteurs parmi lesquels je me range. Mais quel danger cela présente-t-il? le nombre des corrections à faire sera moindre, et voila tout.

Je reconnais qu'il n'est pas toujours très facile de se procurer un exemplaire de l'arithmetica logarithmica de Vlacq, et que la rareté de l'ouvrage en rend le prix assez élevé. Tout fois on peut y suppléer avantageusement à l'aide du "Thesaurus logarithmorum completus" de Véga, qui est un ouvrage fort estimable, moins rare et par suite moins cher que le premier. J'ai composé aussi un errata pour les tables de Véga. J'en joins la copie à cette note, et j'en autorise biens volontiers la publication. Mon édition du Thesaurus porte la mention, "Leipzig, 1794."

La difficulté de consulter le 4^e volume des annales de l'Observatoire de Paris, n'est pas sérieuse pour des villes comme Edimbourg,

Londres, &c., qui possédent des Bibliothèques publiques et des établissments scientifiques de premier ordre. La copie de mon errata ne demande que quelques heures.

II. J'arrive maintenant au grandes tables du Cadastre et c'est a leur sujet, surtout, que j'éprouve le besoin de rectifier plusieurs des interprétations de M. Sang.

Les observations présentées par M. Le Verrier, dans le séance de l'Académie des Sciences de Paris, en date du 17 Mai 1858, ne résultent pas d'un examen personnel auquel se serait livré le savant directeur de l'Observatoire, mais des conférences que j'ai eu l'honneur d'avoir avec lui. Ainsi que je l'ai dit dans la note présentée à la séance de 21 Mai, "M. Le Verrier avait bien voulu mentionner mes recherches dans la dernière séance." Donc les doutes élevés par ce savant sur la véritable *originalité* des calculs en quelques endroits, n'ont ni plus ni moins de portée que ceux que j'ai exprimés moi-même dans cette séance de 21 Mai, et n'infirment en quoi que ce soit ma conclusion, que je maintiens plus firmement que jamais: "Les tables du Cadastre, comme toutes les œuvres humaines, ne sont donc pas parfaites. Elles ne le sont, ni dans l'exécution, ni, peut-être, dans les détails de la conception. Cependant, elles surpassent de beaucoup, non seulement en étendue mais encore et surtout en correction toutes les tables qui les ont précédées, et les tables plus modernes qui ne lui ont pas été comparées avant la publication."

Le 2^e paragraphe, pag. 12, de la brochure qui m'a été adressée, paragraphe, que je ne reproduis pas à cause de son développement, contient une erreur capitale que je n'ai pas conscience d'avoir fait naître. M. Sang dit qu'il y a un troisième exemplaire des tables "third copy," que avait été laissé à Prony à titre de minute. Je n'ai jamais rien avancé de semblable. Il n'existe en fait que deux exemplaires "two copies" manuscrits des grandes tables du Cadastre. L'introduction de la notice que j'ai publiée dans le tome IV des annales de l'Observatoire, ne laisse aucun doute à cet égard. On y voit comment j'ai été amené, après de longues recherches, a découvrir l'un des *deux* exemplaires que l'on croyait perdu.

En présence de ce fait, il serait inutile que je cherchâsse à combattre les conséquences que tire M. Sang de l'existence d'une transcription: cette transcription est purement imaginaire.

III. M. Edward Sang, mû évidemment par l'unique désir d'obtenir des tables logarithmiques parfaites, met le public savant en défiance contre la valeur réelle des tables du Cadastre. Suivant lui, ces tables n'offriraient de garanties sérieuses que si la méthode suivie était bonne en principe, si elle avait été fidèlement exécutée, et si les résultats étaient sincèrement produits.

Sur ces trois chefs "on these three heads," M. Sang se sert de moi comme d'un bélier pour démolir l'édifice construit par Prony, et il croit avoir si bien réussi qu'il lui parait inutile de récapituler les critiques partielles aux quelles il s'est livré. Je pense qu'il aurait conçu une opinion tout autre, s'il lui avait été donné de consacrer quelques années à l'étude d'un travail qui se résume par 19 volumes in folio.

Pour se rendre compte dans tous ses détails de la partie mathématique de la méthode, il faudrait lire l'exposé qu'en a fait Prony dans le volume qui sert d'introduction aux tables. Quoique le mémoire ne soit pas très volumineux, et que j'en aie personnellement pris copie, je n'ai pas trouvé d'imprimeur qui consentit à courir les risques de l'impression. Je me suis alors borné à profiter de la place très honorable, mais restreinte, que M. Le Verrier voulait bien me donner dans l'important receuil qu'il publie, et j'ai cherché, par mon travail personnel, à mettre en saillie tout ce qui m'a paru capital dans l'œuvre de Prony.

Serait-il opportun aujourd'hui de répondre en détail aux critiques de M. Sang? Je l'ignore. En tout cas, le temps me manquerait pour faire une réponse complète. Je dirai seulement que le mystère, "mystery," qui lui parait résulter de l'insuffisance de la collation operée par MM. Letellier et Guyétant sur les tables de Briggs, n'en est pas un pour quiconque a recouru aux sources originales.

IV. Bien que Briggs ait dû à Napier, non seulement l'idée mêre des logarithmes, mais même l'idée de leur construction dans le système dont la base est 10, système dont on l'a fort à tort considéré comme l'inventeur et auquel on a donné son nom, j'apprécie à un très haut degré le travail de ce collaborateur de Napier, mais l'étude de l'arithmetica logarithmica m'a permis de reconnaitre des erreurs dans l'établissement même des bases du calcul, et m'a expliqué sur abondamment les fautes qui entachent le grand ouvrage de 1624.

La table de la page 10, "numeri continue medii inter denarium et unitatem" renferme des erreurs, ainsi que cela résulte d'une table analogue, et plus étendue, calculée par Callet.

La table de la page 32, "tabula inventioni logarithmorum inserviens" est également fautive, d'après les travaux de Léonelli et de M. Houël.

On ne peut donc attribuer aux calculs de Briggs, qui reposent sur des bases entachées de quelques erreurs, aucune supériorité sur les calculs effectués au bureau du Cadastre.

Cependant M. Edward Sang va plus loin. Il attaque la méthode des différences mise en usage par Prony, et parait lui préférer les procédés de Briggs, ou ceux que lui-même a récemment employés. Je ne parlerai pas de ces derniers qui me sont inconnus; mais, ayant longuement étudié les procédés de Briggs, et ayant pratiqué moi-même la méthode des différences pour calculer à 7 decimales des tables de logarithmes d'addition et de soustraction, je me crois en droit de combattre les critiques élevées contre cette dernière méthode.

La critique principale de M. Sang est enfermée dans la phrase suivante: "Also an error in the determination of the first difference of the sixth order is augmented 82 472 326 300 times in the final logarithm." En d'autres termes, quand on veut calculer des logarithmes à 14 décimales, en faisant usage de 6 ordres des différences, l'approximation étant portée pour le 1er ordre à 16 décimales, le 2^{e} à 18, le 3^{e} à 20, le 4^{e} à 22, le 5^{e} à 24, et le 6^{e} à 26, l'erreur résultant de l'incertitude sur la valeur de 26^{e} chiffre décimal est multipliée après 200 termes par 82 472 326 300.

Pour voir nettement ce qu'il en est, faisons usage des signes algèbriques. En donnant aux lettres le sens que je leur ai assigné dans mon mémoire inséré au tome IV des annales de l'Observatoire, on a pour la détermination du logarithme final, u_p en fonction du logarithme initial u_0 et des differences successives de u_0 jusqu'au 6^{e} ordre,—

$$u_p = u_0 + p\Delta u_0 + p\frac{p-1}{2}\Delta^2 u_0 + p\frac{p-1}{2}\frac{p-2}{3}\Delta^3 u_0$$

$$+ \dots \frac{p-3}{4}\Delta^4 u_0 + \dots \frac{p=4}{5}\Delta^5 u_0 + \dots \frac{p-5}{6}\Delta^6 u_0 .$$

Si l'on désigne par E_0, E_1, E_2, E_3, E , ... , la plus grande

erreur que comporte le calcul de u_0, Δu_0, $\Delta^2 u_0$, ... on a, en assignant à toutes les erreurs le même signe, pour la plus grande erreur possible sur le calcul de u_p,

$$E_p = E_0 + pE_1 + p\frac{p-1}{2}E_2 + \ldots\ldots p\frac{p-1}{2}\frac{p-2}{3}\frac{p-3}{4}\frac{p-4}{5}\frac{p-5}{6}E_5 .$$

$p = 200$, de sorte que les coefficients successifs ont les valeurs suivantes :

$$200 = 2.10^2,\ 19\,900 \angle 2.10^4,\ 1313\,400 \angle 1{,}4.10^6$$
$$64\,684\,950 \angle 1.10^8,\ 2535\,650\,040 \angle 3.10^9,$$
$$82\,408\,626\,3000 \angle 1.10^{11}:$$

Ainsi, la plus grande erreur finale sur la valeur de u_{200}, déterminée par la méthode des différences, sera moindre que

$$0{,}0^{14}5 + 0{,}0^{13}1 + 0{,}0^{14}7 + 0{,}0^{14}5 + 0{,}0^{14}2 + 0{,}0^{15}5 = 0{,}0^{13}395\,;$$

en sorte que par le seul fait de la répétition des erreurs commises sur les éléments du calcul, l'erreur totale ne pourrait s'élever à plus de 4 unités du 14^e ordre décimal. On voit combien est fantasmagorique le chiffre 82 472 326 300 (d'ailleurs inexact), qui est donné par M. Sang.

En fait, l'erreur s'élève en réalité plus haut dans les tables du Cadastre, mais cela a lieu par suite des différences omises. Aussi ai-je dit que les logarithmes ont été calculés avec 14 décimales, mais en vue d'avoir seulement 12 décimales exactes, et cette correction est presque absolument assurée.

V. Loin de moi l'idée d'inculper les intentions de M. Edward Sang: je suis convaincu qu'il n'a eu d'autre but que la recherche de la vérité. Le libéralisme scientifique de l'Angleterre est trop connu, et s'est manifesté, il y a quelques anneés, d'une manière trop honorable par la publication des tables de la lune de Hansen, pour qu'on puisse supposer qu'un savant, appartennant à cette nation, cherche de propos délibéré, à discréditer une grande œuvre Française, sur laquelle il est peu on mal renseigné.

Il n'a pas dépendu, et il ne dépend pas encore de moi de porter plus de lumières sur un sujet qui m'a occupé pendant plusieurs années. En 1857, j'ai présenté à l'Académie des Sciences de Paris un mémoire fort étendu sur la théorie des logarithmes, la construc-

tion et l'usage des tables logarithmiques. Dans ce travail, j'ai passé en revue tout ce qui a été fait d'important depuis Napier jusqu'à nos jours. Notamment, j'ai fait connaître avec beaucoup de détails l'œuvre de Briggs, et le monument élevé, sons la direction de Prony, par le bureau du Cadastre. Ce serait la matière d'un volume in 4° de 200 pages environ. Je n'ai trouvé personne qui consentit à supporter les frais d'impression.

J'extrairai volontiers de mon travail tout ce qui pourra intéresser les savants, et, pour le prouver, je ne crois pouvoir mieux faire que de joindre à cette notice l'errata que j'ai formé pour le "Thesaurus Logarithmorum Completus de Vega." Je n'ai pas souvenance de l'avoir déja publié.

J'ai composé aussi un errata pour l' "Arithmetica Logarithmica de Briggs," qui contient environs 300 (trois cents) articles; mais sa publication devrait être précédé de quelques détails qu'il m'est impossible de donner aujourd'hui. Je ferai remarquer seulement que M. Sang ne parait pas avoir lu, dans mon mémoire inséré au tome IV des annales de l'Observatoire de Paris, la phrase où j'indique dans quelle mesure étroite la collation des tables de Briggs avec les tables du Cadastre a été faite par MM. Letellier et Guyétant: "La collation operée par MM. Letellier et Guyétant ne porte réellement que sur 12 chiffres. Elle aurait pu être étendue à 14 chiffres pour les dix milles premièrs nombres, dont les logarithmes ont été calculés au bureau de Cadastre avec 19 décimales." Tout le mystère consiste donc en ceci. MM. Letellier et Guyétant n'étaient pas des calculateurs de la 2[e] section; et il se sont bornés à comparer le travail de Briggs avec celui qui avait été fait par les calculateurs du bureau de Cadastre—qui, comme eux, appartenaient à la 3[e] section.

On sait que Legendre a publié, dans son traité des fonctions elliptiques, les logarithmes à 19 figures, tels qu'ils résultent des calculs faits au bureau du Cadastre, pour les nombres premiers compris entre 1 et 10,000.

L'errata qui suit ne reproduit pas l'errata imprimé à la page XXX du Thesaurus Logarithmorum completus. On suppose que les corrections indiqueés par l'auteur ont déja été faites sur l'exemplaire que le calculateur possède.

ERRATA.

THESAURUS LOGARITHMORUM COMPLETUS, ETC., À GEORGIO VEGA.

Emendatis erroribus ab auctore Semetiposo prius signatis, non nulli infra signati adhuc supersunt.

I.

MAGNUS CANON LOGARITHMORUM VULGARIUM.

Locus corrig.		Error.	Correct.
Log.	558	90	89
	863	8	7
	869	5	4
	10 033	3	2
Diff.	10 032		887
Diff.	10 033		845
Log.	11 003	29	30
Diff.	11 002		724
Diff.	11 003		687

Differentiarum maculæ, brevitatis causâ, haud ultra adscribuntur: attento lectori patent.

Locus corrig.		Error.	Correct.
Log.	11 240	3	2
	15 620	6	5
	17 646	8	9
	17 647	6	7
	17 648	0	1
	17 649	0	1
	20 071	10	09
	20 280	6	7
	20 375	5	4
	20 645	3	2
	20 822	2	1
	20 866	1	0
	21 245	5	4
	21 749	2	3
	21 795	5	4
	21 904	9	8
	22 016	7	6
	22 200	4	5

Locus corrig.		Error.	Correct.
Log.	22 312	2	3
	22 877	1	2
	22 996	2999	3000
	23 274	299	300
	23 492	3	2
	23 820	2	1
	24 156	10	09
	24 626	9	8
	25 173	9	8
	25 524	59	60
	25 586	5	6
	25 707	5	6
	26 004	3	4
	26 188	2	3
	26 407	5	4
	26 642	39	40
	26 717	4	5
	26 728	46	26
	27 291	5	4
	27 560	3	2
	27 586	8	9
	27 861	2	3
	27 921	7	6
	28 486	699	700
	28 680	69	70
	29 112	5	6
	29 163	8	9
	29 226	799	800
	29 446	7	8
	29 639	8	7
	29 703	3	2
	30 499	6000	5999

Locus corrig.	Error.	Correct.	Locus corrig.	Error.	Correct.
Log. 30 502	8	7	Log. 48 845	40	39
30 728	1	2	48 980	9	8
31 001	2	1	49 047	6	5
31 627	5	6	49 409	1	2
31 653	6	8	50 211	9	8
31 735	6	7	50 414	1	0
31 817	79	80	50 601	7	6
31 919	8	7	50 828	3	2
32 111	5	6	50 937	1	0
32 633	9	10	50 996	5	4
32 672	5	4	51 037	3	2
33 071	23	27	51 096	2	1
33 370	6	7	51 175	4	3
34 037	6	7	51 299	3	2
34 162	4	3	51 388	5	4
34 358	4	3	51 389	7	6
34 664	1	0	51 606	1	0
34 702	4	5	51 607	6	5
34 734	7999	8000	51 820	7	6
35 053	8	9	51 915	4	3
35 298	7	8	52 064	2	3
38 051	9	7	52 533	8	7
38 277	1	2	52 565	8	7
38 321	7	6	52 587	8	7
38 783	3	2	52 620	8	7
39 227	4	5	52 792	3	4
39 802	5	4	52 823	7	6
39 839	7	6	52 986	2	1
40 108	2	3	53 647	8	7
40 127	19	20	53 868	5	4
40 966	6	7	54 026	3	2
41 156	5	6	54 145	1	0
41 227	2	3	54 273	4	3
41 385	6	5	54 419	70	69
42 584	1	2	54 708	3	2
44 121	40	39	54 825	4	3
44 822	2	3	55 010	50	49
45 060	3	4	55 115	8	7
45 231	5	6	55 313	9	8
45 238	3	2	55 618	768	678
45 474	5	4	57 089	8	7
45 549	8	7	57 202	7	6
45 571	8	7	57 486	6	5
45 697	7	6	57 751	8	7
45 725	2	1	58 081	2	1
45 755	6	7	58 214	6	5
46 073	9	8	58 223	2	1
47 162	40	39	58 301	1	0
47 476	1	2	58 858	7	6
48 305	5	4	59 007	1	0
48 614	6	7	59 488	4	3
48 626	8	7	P. 173, col. 0 777	Lin. 5[a]	Lin. 6[a]

Locus corrig.		Error.	Correct.	Locus corrig.		Error.	Correct.
Log.	60 096	2	3	o g.	73 571	2	1
	60 401	8	9		73 655	9	8
	60 487	2	1		74 527	6	5
	60 704	1	2		74 723	8	7
	60 794	2	1		74 733	5	4
	61 011	4	3		74 932	5	4
	61 157	4	3		74 941	40	39
	62 038	5	4		75 149	9	8
	62 131	7	6		75 386	2	1
	62 173	7	6		75 395	6	5
	62 257	4	3		75 560	3	2
	62 273	4	3		75 562	4	5
	62 933	50	49		75 590	4	3
	63 183	9	8		75 613	4	3
	63 357	50	49		75 841	8	7
	63 887	1	0		75 953	4	3
	64 086	5	4		77 047	2	1
	64 639	1	0		77 437	6	5
	64 661	4	8		77 663	7	6
	64 993	40	39		77 944	6	5
	65 143	1	0		78 079	5	4
	65 185	8	9		78 259	2	1
	65 311	5	4		79 447	5	4
	65 659	1	0		79 467	1	0
	65 946	2	3		79 666	20	19
	66 187	7	6		80 060	7	8
	66 239	4	3		80 062	8	9
	66 423	7	6		80 063	2	3
	67 399	30	29		80 090	6	7
	69 311	7	8		81 212	60	59
	69 457	3	2		81 460	8	7
	69 477	5	4		82 951	60	59
	69 988	2	1		82 991	7	6
	70 019	40	39		83 693	6	5
	70 040	3	4		83 803	8	9
	70 043	1	0		85 651	9	8
	70 066	7	8		85 810	19	20
	70 599	6	5		86 688	3	4
	71 140	9	8		86 708	90	89
	71 306	9	8		86 898	0	1
	71 569	0	1		87 634	3	2
	71 653	3	4		89 182	7	6
	71 764	6	5		89 185	6	7
	72 103	9	8		90 625	5	6
	72 675	5	4		91 086	8	7
	73 046	90	89		91 087	7	6
	73 059	4	5		93 155	1	2
	73 286	2	3		93 498	0	1
	73 303	90	89		96 981	80	79
	73 404	6	5		97 674	5	6
	73 501	9	8		98 336	5	6
	73 570	1	0		98 337	49	50

Locus corrig.	Error.	Correct.	Locus corrig.	Error.	Correct.
Log. 98 338	3	4	Log. 98 772	8	7
98 339	6	7	98 936	7	8
98 340	39	40	98 966	4	3
98 341	1	2	99 926	2	1
98 342	3	4	100 330	3	2
98 345	6	7			
98 346	6	7	Wolframii tabula Logarithmorum naturalium.		
98 348	5	6			
98 350	2	3	Log. 1 099	0021 1	0021 5
98 352	7	8	7 853	9676	9686
98 353	4	5			
98 356	2	3			
98 357	7	8	Magnus Canon Logarithmorum vulgarium trigonometricus.		
98 358	2	3			
98 359	6	7			
98 360	0	2	Log.tan. 0° 30′ 45″	101	201
98 362	6	7	Cotg. 0° 30′ 45″	899	799
98 365	2	3			
98 366	3	4	Aliæ maculæ posterius signabuntur.		
98 367	4	5			

PARIS, *le* 29 *mars* 1875.

F. LEFORT.

Translation.

1. Observations relative to Mr Edward Sang's "Remarks on the Great Logarithmic and Trigonometrical Tables calculated in the Bureau du Cadastre under the direction of Prony," published in the Proceedings of the Royal Society of Edinburgh, Session 1874–1875, by M. F. Lefort, Inspecteur General des Ponts et Chaussées, Corresponding Member of the Academy of Sciences of Naples.

Mr Edward Sang, in the above cited article, makes most flattering mention of the works which I have published on the subject of Logarithms, and particularly on the great operations performed in the end of the last century under the direction of Prony. I cannot but thank him, yet without wishing to intervene in any way in the controversy between him and the editor of the scientific periodical "Nature." But I owe it to him, as well as to the honourable Royal Society of Edinburgh, to give explanations on

several points of fact and of doctrine which he regards as deducible from my writings; writings which he has misinterpreted, doubtless from an incomplete knowledge of the French language. In preparing the following note I am aware that I am exposed to a danger of the same kind, and therefore count upon Mr Sang's indulgence, as he may assuredly count upon mine.

I. Mr Edward Sang does not admit that Vlacq's Table corrected by help of my *errata* can supply the place of the new tables which he proposes to print. He argues that Vlacq's Tables can only be had at a great price, and that the 4th volume of the "Annales de l'Observatoire de Paris," is not always easily obtainable; and also that there is not a complete agreement among the different copies of Vlacq's work. In support of this thesis he quotes the following phrase, which should be found on the 64th page of Taylor's Tables:— "In about 100 copies; in about 200 copies; doubtful whether a few copies are erroneous or not; in about half the impression; only in one copy; and so on."

I have a copy of Taylor's Tables, published in 1792 at London, under the care of Maskelyne. Therein I find at page 64 an *errata* with this notice, very different from the preceding:—"Errata of the Logarithmic Tables which affect only part of the impressions of the sheet, and have been corrected by the printer since the impression, except any may have escaped correction through inadvertence."

Have there been several editions of Taylor's Tables? I know nothing of it. But the preface to the edition just mentioned shows that the publication was made for the first time under Maskelyne's care. Any way the two quotations seem to me to apply exclusively to Taylor's work, and to have no reference to that of Vlacq.

The *errata* which I have given in vol. 4 of the "Annales de l'Observatoire," refer to the Arithmetica Logarithmica par Adrian Vlacq, Goudanum, Goudæ 1628, Petrus Rammasenius, and not to any spurious copies.

As to the hypothesis of the types drawn out in the working of the "inking dabber," and misplaced by the pressman; I cannot imagine how it could be, because it supposes the complete neglect of the most elementary and usual verifications; an omission much

the less likely since Vlacq, the author of these valuable tables, had been a printer, and was the immediate predecessor of Petrus Rammasenius.

I willingly admit that, in the course of successive impressions, the copies of Vlacq may have been made more correct; it is thus possible that a particular copy may not contain precisely all the errors indicated by Vlacq, Vega, and other authors, among whom I may count myself. But what danger is there from that? there are fewer corrections to be made; that is all.

I admit that it is not always very easy to procure a copy of the "Arithmetica Logarithmica" of Vlacq, and that the scarcity of the book enhances its price. However, the want may be advantageously supplied by the Thesaurus Logarithmorum Completus of Vega, a most estimable work, not so rare and therefore not so costly as the other. I subjoin a copy of a list of errors in Vega which I have made, and of which I willingly authorise the publication; my copy of the Thesaurus has the legend Leipzig, 1794.

There can be no serious difficulty in consulting the 4th volume of the "Annales de l'Observatoire de Paris," in such towns as Edinburgh, London, etc., where there are public libraries and scientific establishments of the first order. The copying of my errata is the matter of a few hours.

II. I come now to the Great Tables of the Cadastre, on which subject chiefly I find it necessary to rectify several of Mr Sang's interpretations.

The observations made by M. Le Verrier at the meeting of the Academy of Sciences of Paris, of date 17th May 1858, were not the results of a personal examination made by the Director of the Observatory, but of the conferences which I had the honour of having had with him. As I have said in the note presented at the Meeting of the 24th May:—" M. Le Verrier has been kind enough, at the previous meeting, to mention my researches." Hence the doubts expressed by this philosopher as to the true originality of the calculations in some places, have neither more nor less weight than those which I myself have expressed at this meeting of the 24th May, and do not weaken in the least the conclusion which I maintain more firmly than ever:—" The Tables of the Cadastre,

like all human works, are not then perfect; they are so neither in the execution nor perhaps in the details of the conception; nevertheless, they greatly surpass, not only in extent, but yet and above all in correctness, all the tables which have preceded them, as well as the more modern tables which have not been compared with them before publication."

The second paragraph, p. 12, of the pamphlet sent me, which on account of its length I do not quote, contains a capital error for which I cannot admit that I have given cause. Mr Sang says that there is a third copy of the tables which had been allowed to Prony by way of minutes. I have never said anything of the kind. There only exist, in fact, two manuscript copies of the Great Tables of the Cadastre. The introduction and notice which I have published in vol. 4 of the Annals of the Observatory leave no doubt on that subject. It may be seen therein how, after long researches, I was led to discover that one of the two which was believed to have been lost.

It is, therefore, unnecessary for me to seek to controvert the consequences which Mr Sang has drawn from the existence of an imaginary transcription.

III. Mr Edward Sang, evidently led by the sole desire to obtain perfect logarithmic tables, would have the learned world to mistrust the Cadastre Tables. According to him, these afford no serious guarantee that the principle of the method was good, that these principles were faithfully carried out, or that the results were sincerely given. On these three heads Mr Sang uses me as a battering ram to demolish the edifice erected by Prony, and thinks he has so well succeeded that it was unnecessary to recapitulate the special criticisms which he had made. I think that he would have formed quite a different opinion if he had been privileged to spend years in the study of a work which fills nineteen folio volumes.

In order to give an account of all the mathematical details of the method, it would be necessary to read Prony's explanation in the (manuscript) volume forming the introduction to the tables. Although this memoir be not exceedingly voluminous, and although I have personally made a copy thereof, I have not found a printer willing to run the risk of the impression, and have, therefore, been confined to the honourable but restricted space kindly given by

M. Le Verrier in the annals which he publishes; and I have endeavoured by my own exertions to exhibit all of Prony's work that appeared to me to be most important.

I do not know that it would be opportune at present to reply in detail to Mr Sang's criticisms, and need only say, that the mystery which he thinks to result from the insufficient collation of Briggs' Tables by MM. Letellier et Guyétant is no mystery to those who have had recourse to the original sources.

Although it be true that Briggs owes to Napier not only the fundamental idea of logarithms, but also that of the system computed according to the basis 10, of which system Briggs has without reason been held as the inventor, and to which his name has been attached, I appreciate in the highest degree the work of this fellow labourer with Napier. But the study of the Arithmetica Logarithmica has led me to discover errors in the fundamental basis of the calculation, and has superabundantly explained the faults which mar the great work of 1624.

The table in page 10, "Numeri continue medii inter denarium et unitatem," contains several errors, as is seen from an analogous and more extensive table calculated by Callet.

The table on page 32, "Tabula inventioni logarithmorum inserviens" is equally faulty, according to the works of Leonelli and of M. Houël.

We must not then attribute to the calculations of Briggs, founded on pages containing various errors, any superiority over those executed in the Bureau du Cadastre.

For all that, Mr Edward Sang goes farther; he attacks the method of differences made use of by Prony, and seems to prefer to it the processes followed by Briggs, or those which he himself has recently employed. I say nothing about these last, which are unknown to me; but having for long studied Briggs' processes, and having myself practised the method of differences while computing to 7 decimals tables of logarithms of sums and differences, I believe myself to be in a position to repel the attacks on this latter method.

Mr Sang's principal objection is contained in the following phrase:—"Also an error in the denomination of the first difference of the sixth order is augmented 82 472 326 300 times in the final

logarithm." In other words, when we wish to calculate logarithms to 14 decimals, making use of 6 orders of differences, the approximation being carried for the 1st order to 16, for the 2d to 18, the 3d to 20, the 4th to 22, the 5th to 24, and the 6th to 26, the error resulting from an uncertainty in the 26th figure is multiplied after 200 terms by 82 472 326 300.

To see exactly the state of matters, let us make use of algebraic signs. Giving to the letters the meaning which I have assigned to them in my memoir inserted in the 4th volume of the "Annales de l'Observatoire," we have for the determination of the final logarithm u_p in terms of the initial logarithm and of the successive differences up to the sixth order,

$$u_p = u_0 + p\Delta u_0 + p\frac{p-1}{2}\Delta^2 u_0 + p\,\frac{p-1}{2}\,\frac{p-2}{3}\Delta^3 u_0$$

$$+ \ldots \frac{p-3}{4}\Delta^4 u_0 + \ldots\ldots \frac{p-4}{5}\Delta^5 u_0 + \ldots\ldots \frac{p-5}{6}\Delta^6 u_0 \,.$$

If we denote by E_0, E_1, E_2, ... the greatest error which arises in the calculation of u_0, Δu_0, Δu_0 . . . . we have, on giving the same sign to all the errors, in order to obtain the greatest possible error in the result.

$$E_p = E_0 + pE_1 + p\,\frac{p-1}{2}E_2 + \ldots\ldots p\frac{p-1}{2}\,\frac{p-2}{3}\,\frac{p-3}{4}\,\frac{p-4}{5}\,\frac{p-5}{6}E_6 \,.$$

putting each error in the differences at $\frac{1}{2}$ of a unit in its own last place, and making $p = 200$, the successive coefficients, have the following values—200 , 19900 , 13 13400 , 64 684 950 , 2 535 650 040 , 82 408 626 300 .

Thus, the greatest final error in the value of u_{200} determined by the method of differences must be less than 0·5 + 1·0 + 0·7 + 0·5 + 0·2 + 0·05 = 3·95 in the fourteenth place; so that by the simple repetition of the errors made in the elements of the calculation, the total error can never rise to more than four units in the 14th decimal place. We see thus how fantasmagoric is the number 82 472 326 300 (inaccurate besides) which is given by Mr Sang.

In point of fact, the error really rises higher than this in the Cadastre tables, but that is because of the differences omitted.

Thus, I have said that the logarithms have been calculated with 14 decimals, but with the view only of having 12 exact; and this degree of accuracy is almost absolutely secured.

V. Far be it from me to entertain the idea of blaming the intentions of Mr Edward Sang. I am convinced that he has had no other desire than to reach the truth. The scientific liberality of England is too well known, and has recently been too well shown, by the publication of Hansen's Lunar Tables, to allow us to suppose that a savant belonging to that nation would deliberately seek to discredit a great French work, concerning which he has been ill-informed.

It has not depended, and it will not depend on me, to throw more light on a subject which has occupied me for several years. In 1857 I presented to the Academy of Sciences of Paris a very extensive memoir on the Theory of Logarithms, on the construction, and on the use of Logarithmic Tables. In this work I have reviewed everything important that has been done from Napier's down to our times. Notably, I have explained with many details the work of Briggs, and the monument erected under the direction of Prony by the Bureau du Cadastre. It would make a quarto volume of some 200 pages; I have found no one willing to bear the expense of the impression.

I would willingly extract from my work anything that would interest the learned; and to prove this I do not think I can do better than annex to this note the *errata* which I have compiled for "Vega's Thesaurus Completus." I have no recollection of having published it before.

I have prepared also an *errata* for Briggs' "Arithmetica Logarithmica," which contains about 300 entries, but its publication would need to be accompanied by some details which I am just now unable to give. I would remark only, that M. Sang does not seem to have read in my memoir inserted in tome IV. des Annales de l'Observatoire de Paris the passage in which I point out the limited extent of the comparison of Briggs' tables with those of the Cadastre made by MM. Letellier et Guyétant. "The comparison made by MM. Letellier et Guyétant extended only to 12 figures. It might

have been extended to 14 figures for the first ten thousand numbers, whose logarithms had been computed to 19 places at the Bureau du Cadastre." All the mystery lies here. MM. Letellier et Guyétant were not calculators of the second section, and they confined themselves to the comparison of the work of Briggs with that which was done by the computers in the Bureau du Cadastre, who, like them, belonged to the third section.

We know that Legendre has published in his "Treatise on Elliptic Functions" the logarithms of all prime numbers from 1 to 10,000, as obtained from the calculations made at the Bureau du Cadastre.

The following *errata* does not contain the errors printed at page xxx. of the Thesaurus Logarithmorum Completus; it is taken for granted that the errors pointed out by the author have been already corrected on the computer's copy.

Reply to M. Lefort's Observations. By Edward Sang.

From M. Lefort's opening sentence it appears that he had only recently received the copy of my remarks which had been posted to him on 22d December. Perhaps on this account M. Lefort has been hurried in the perusal of my paper, and so has fallen into several mistakes as to my meaning. These will be apparent to any one who peruses the writings, and I shall pass them over entirely, confining myself to the very few points which are essential to the subject in hand. The only extraneous matter to which I shall allude is this, that while M. Lefort has obviously and justly been desirous of upholding the dignity of the Grandes Tables du Cadastre, he has, in the true spirit of an inquirer after truth, clearly and faithfully exhibited even those points which press most sorely on his own position.

While disclaiming any intention to enter into the controversy opened by "Nature," he at once plunges into it in support of the thesis enunciated by the *non nemo* of that periodical—"Almost all the errors found by Mr Sang by means of this table are among those there given by Lefort, and any one who chooses can, without much expenditure of trouble, render his copy of Vlacq all but free

from error—*much more accurate than any new table could* POSSIBLY *be.*" In opposition to this gigantic absurdity, I had pointed out the well recognised danger arising from the use of moveable types: M. Lefort denies and yet admits this danger in one sentence:— "J'admets bien qu'à la suite des tirages successives on ait pu rendre plus corrects les exemplaires de Vlacq, mais je n'admets pas qu'on les ait rendus moins corrects." It is enough for my argument that two copies may differ. In the supplementary table to the Errata of Briggs, M. Lefort supplies a strong corroboration of what I advance. The logarithm of 2087 is therein stated as 9.. instead of 952. The two figures 52 had been drawn out or been broken while the copy "Sainte Geneviève" was being printed; in my own copy they are correctly given.

In order to sustain this dictum of "Nature," we have to suppose the Tables du Cadastre, which were the basis of Lefort's comparison, to be absolutely correct. Now, while composing the remarks made at the first meeting of the Session, I had only access to M. Lefort's papers in the Comptes Rendus; but through his great kindness I am now in possession of a copy of his most valuable paper inserted in the Annales de l'Observatoire de Paris, and am thereby enabled much more satisfactorily to explain the defects of Prony's mode of procedure because an example of the actual work is therein given.

The design was to compute the successive differences of the logarithms, carrying the decimals two places further at each step; and by the summation of these to obtain 200 terms of the logarithmic progression. An error of unit in the last place of each of these differences will produce an effect on the final term, according to the following scale:—

1st,	2·00
2d,	1·99 00
3d,	1·31 34 00
4th,	·64 68 49 50
5th,	·25 35 65 00 40
6th,	·08 24 08 62 63 00

making a total, if all the errors should happen to be in one way, of

6·2862. Wherefore, if each difference have been computed true to the nearest last figure, the maximum error arising from this mode of calculation is 3·1431. M. Lefort, taking into account the maximum possible error in the first logarithm, makes it 3·95, or say four units in the last place.

All this looks exceedingly well, but has not the slightest reference to the matter in hand. In order to obtain such a miserable degree of precision, we have the labour of computing the first difference of each order, and then the toil of writing 6 times 12 times 200, or 14 400 unnecessary figures; for, to make M. Lefort's formula applicable, each difference of each order must be carried to the 26th place.

Prony did not use the method of differences; he used a method of vitiated differences. To show the nature of the vitiation, I transcribe a few lines of the actual work from M. Lefort's example, which, belonging to an advanced part, has only differences of the fourth order.

Nombres.	Logarithmes.	Δ^1	Δ^2	Δ^3	Δ^4
100 800	00846 05821 0951	43084 5568·17	4274 19·79	848·03	2·52
801	00846 48405 6514	43084 1288·97	4274 11·81	848·00	2·52
802	00846 91489 7803	43083 7014·86	4274 02·83	847·97	2·52
803	00347 34573 4818	43083 2740·83	4273 94·85	847·94	2·52

Here we see that the differences, though computed true to the last figure, are only used to the second preceding figure; thus 2·52 is read 3, and the possible error is augmented one hundred times. But this is not all; the difference of any particular order only comes to affect those of lower orders by the accident of some of the to be rejected figures being more or less than 50; so that the final effect cannot be made the subject of calculation. I find nowhere any attempt to estimate the effect of this systematic vitiation, and shall endeavour to supply the want by taking two extreme imaginary cases. In the first case I shall assume each of the initial differences to be 0·49. Proceeding with these according to the method of Prony, we find

p	u	Δu	$\Delta^2 u$	$\Delta^3 u$	$\Delta^4 u$	$\Delta^5 u$	$\Delta^6 u$
0	0	0·49	0·49	0·49	0·49	0·49	0·49
1	0	0·49	0·49	0·49	0·49	0·49	0·49
	...	...	...	...	...	...	...
200	0	0·49	0·49	0·49	0·49	0·49	0·49

giving for u_{200} the value 0, whereas if computed according to the method of successive differences, the result is $u_{200} = 308$.

If, however, we augment the sixth difference by two units in its last place, leaving the other differences unchanged, we get

p	u	$\Delta^1 u$	$\Delta^2 u$	$\Delta^3 u$	$\Delta^4 u$	$\Delta^5 u$	$\Delta^6 u$
0	0	0·49	0·49	0·49	0·49	0·49	0·51
1	0	0·49	0·49	0·49	0·49	0·50	0·51
2	0	0·49	0·49	0·49	0·50	0·51	0·51
3	0	0·49	0·49	0·50	0·51	0·52	0·51
4	0	0·49	0·50	0·51	0·52	0·53	0·51
5	0	0·50	0·51	0·52	0·53	0·54	0·51
6	1	0·51	0·52	0·53	0·54	0·55	0·51
50	45	0·95	0·96	0·97	0·98	0·99	0·51
100	95	1·45	1·46	1·47	1·48	1·49	0·51
150	190	2·41	2·43	2·45	2·47	1·99	0·51
195	327	3·80	3·84	3·78	3·37	2·44	0·51
196	331	3·84	3·88	3·81	3·39	2·45	0·51
197	335	3·88	3·92	3·84	3·41	2·46	0·51
198	339	3·92	3·96	3·87	3·43	2·47	0·51
199	343	3·96	4·00	3·90	3·45	2·48	0·51
200	347	4·00	4·04	3·93	3·47	2·49	0·51

Thus a change of two units in the last place of the 6th difference has caused a change of 347 in the value of u_{200}; whereas, if it had been computed by the method of differences, the change would only have been ·164, and thus the number which M. Lefort has characterised as "phantasmagorique" has yet to be augmented more than two thousand times, and it is possible that this egregiously absurd mode of proceeding may cause an uncertainty of three units in the twelfth place; also it cannot be predicated that this actually exemplified error is the maximum one.

I had treated as a mystery the fact that MM. Letellier et

Guyêtant had not noticed the numerous last place errors in the Arithmetica Logarithmica. In regard to this I now find in M. Lefort's paper inserted in the Annales de l'Observatoire the following statement:—"The comparison made by MM. Letellier et Guyétant was really only to 12 figures. It might have been extended to 14 figures for the first 10,000 numbers whose logarithms had been computed to 19 places in the Bureau du Cadastre," from which it seems that the object of the comparison was not to correct Brigg's tables but to verify, in so far, the Cadastre tables themselves.

The only other point to which I would refer is as to my mistake concerning a *third copy.* The explanation is simple. In common with many others, I had understood that the two copies of the great tables were deposited in separate libraries. Having read only the papers in the Comptes Rendus, which contain no notice whatever of the loss and recovery of one of these copies, nor of the important service rendered by M. Lefort in that recovery, I naturally regarded the *presentation* to the Academy as that of a third copy. The detail of these matters, interesting to all classes of computers, is contained in the Annales de l'Observatoire, a sectional work consulted by only a limited class. From this paper we learn that one of the two copies, so like as to be hardly distinguishable, had been long amissing, its whereabouts unknown, until M. Lefort, by untiring perseverance, traced it to the possession of the Heirs of Prony, to whom it had been allowed by way of minutes, "Cet exemplaire avait été laissé à Prony à titre de minute." That is to say, the Director had taken away one half of the result of this enormous labour, lessening greatly the value of the remaining half by depriving it of the means of verification; and that the so-called presentation was only the restitution of what should never have been taken away.

I crave leave to add one word in regard to the nineteen-place table. On comparing the logarithms of primes from 1163 to 10007 as given by Legendre in his "Exercises de Calcul Integral," Tome III., with my own to twenty-eight places, it is found that, for primes above 1900, hardly a logarithm is true to the nineteenth place; so much so, that to make a list of the errors would be to

make a list of all the primes. The only logarithms above 1900 truly given are those of 2417, 2879, 2903, 6379, 8599, and 9137; and, with the exception of the logarithm of 9479 which is unit in excess, all those erring by less than 10 are in defect. A list of the corrections exceeding 9 is subjoined.

Numb.	Corrn.	Numb.	Corrn.	Numb.	Corrn.
1303,	− 10	4201,	+ 28	6659,	− 2494
1579,	+ 10	4409,	+ 55	6781,	− 45
2003,	+ 13	5233,	+ 10	6827,	− 25
2011,	+ 12	5273,	+ 10	6883,	+ 30
2203,	+ 55	5813,	− 245	7001,	+ 53
2207,	+ 30	6011,	+ 14	7109,	− 295
2633,	+ 13	6037,	+ 10	8011,	+ 10
3307,	+ 55	6269,	+ 15	8069,	− 494
3863,	+ 25	6521,	+ 14	8353,	+ 12
3923,	+ 10	6581,	+ 14	8819,	+ 31
4007,	+ 19	6619,	+ 29	9403,	+ 15

The only error higher than the sixteenth place is in the logarithm of 4603, which should be 93974 instead of 93924.

From this it is obvious that the mechanical part of the work had been carefully performed, but that the computers had been unskilled in the management of the final figures, so as to prevent the accumulation of small errors. The fact that almost all the errors lie in one direction points to the influence of some definite but erroneous bye rule.

Finally, on examining the list of corrections given in vol. iv. of the "Annales de l'Observatoire," by help of which, according to "Nature," Vlacq is to be made "much more accurate than any new table could possibly be," I find between the narrow limits from 20000 to 30000 two omissions, at 24580 and 26699, and two mis-corrections, at 26188 and 29163, in all of which M. Lefort has been misled by errors of calculation made at the Bureau du Cadastre, as is clear from the subjoined logarithms set down true to the 15th place—

24580	·39058 18785 50435
26188	·41810 23322 49959
26699	·42649 49953 49034
29163	·46483 21978 49968

We must therefore, it seems, be careful lest in correcting Vlacq by help of Prony's calculations, we do not put him wrong where he is right.

Postscript by M. Lefort.

Les erreurs signalées sur le 10^{me} chiffre décimal pour les logarithmes des nombres 24580, 26188, 26699 et 29163 sont moindres qu'une unité du 12^{me} ordre décimal. Or M. Lefort, dans son article sur les tables du Cadastre a prévenu que "le 12^{me} chiffre décimal peut être accidentellement en erreur de près d'une unité," page 26.

Monday, 21st June 1875.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. Note on Electric Resistance of Solutions. By William Durham and P. R. Scott Lang, M.A.

This note contains the results of experiments we have made on the electric resistance of solutions by a method brought under the notice of this Society by Messrs Ewing and M'Gregor, and described in their paper printed in the Transactions, vol. 27, page 51. Our results, so far as they have gone, are as follows:—

1. Resistance of solutions of sodium-chloride, and potassium-chloride, varying in strength from ·002 grains to 4 and 5 grains to 25 cubic inches of water. In these weak solutions the polarization was very little and easily got rid of, and the results satisfactory. On plotting these out in the usual manner, we found the curves described to be hyperbolas, as shown in the diagram, where the ordinates represent the strengths of the solutions and the abscissæ the resistances. Becquerel, in his experiments on this subject, found the hyperbolas to be rectangular for the solutions he used, while Ewing and M'Gregor found theirs not to be rectangular. We find some of our curves to be rectangular and others not. Thus we have—

KCl—not rectangular.

We tried with the same arrangement the resistance of distilled water, and found it to be about 37,000 B.A. units per cubic centimetre; but on carefully distilling water twice, we found the resistance had risen as high as 47,000 B.A. units, showing the great difference the least impurity made.

2. The effects of heat on electric resistance. We experimented on water, sodium-chloride, and potassium-chloride—weak solutions of the two latter. We heated them to about 70° centigrade, and measured the resistance as they cooled. We found, as the temperature fell, the *rate of increase* of resistance increased, and the results, on being plotted, all described rectangular hyperbolas, as shown in the diagram. Since making our experiments we find that Professor Beetz of Munich has been making experiments on the same subject, using zinc electrodes and zinc sulphate, thus avoiding polarization almost entirely. His results and ours agree generally.

3. From some phenomena we noticed we were led to try the effect of varying the strength of the current passing through the solution; and as the result of many experiments we find that, as the strength of the current increases, the resistance seems to diminish. We note the results of two experiments on a weak solution of sodium-chloride and a stronger one of copper-sulphate.

Resistance in Current.	Resistance in Solution.
10 B.A. Units	950 B.A. Units.
100 ,,	1000 ,,
1000 ,,	1150 ,,
10,000 ,,	1390 ,,
10 ,,	132 ,,
100 ,,	138 ,,
1000 ,,	158 ,,
10,000 ,,	187 ,,

We are not prepared as yet to say to what this effect is due. It may be due in some way to the polarization, but we cannot say for certain till we make further experiments. Our thanks are due to Professor Tait for kindly allowing us the use of laboratory and apparatus.

2. On the Circumscribed, Inscribed, and Escribed Circles of a Spherical Triangle. By C. G. Colson, Esq. Communicated by Professor Tait.

In the following paper I propose to investigate expressions for the vector of the following six points of a spherical triangle:—

(1.) Pole of inscribed circle.

(2.) (3.) (4.) Poles of escribed circles.

(5.) Pole of circumscribed circle.

(6.) The orthocentre or intersection of arcs drawn perpendicularly from angles upon the opposite sides.

These vectors will all be found in terms of the vector of the corners of the triangle drawn from the centre of the sphere.

Throughout the investigation α, β, γ will denote the vectors of A, B, C, the corners of triangle ABC, A′B′C′ will represent the polar triangle of ABC (A′ being pole of BC), &c.; α' β' γ' will denote the vectors of its corners; and following the notation usual in spherical trigonometry, a, b, c, A, B, C will denote sides and angles of the triangle; p_1, p_2, p_3, the perpendicular arcs from A, B, C on BC, &c.; R, r, r_1, r_2, r_3 the radii of the circumscribed, inscribed, and escribed circles.

After finding these vectors we proceed to deduce certain well-known results, among others, to find the radius of the circle (analogous to that discovered by Feuerbach in the case of a plane triangle) which touches the inscribed circle and the three escribed circles.

To find the vector of the pole of inscribed circle. Let ρ be the vector (from centre of sphere) of P, the pole of inscribed circle of the triangle ABC. Then we may express ρ as follows:—

$$\rho = x\alpha + y\beta + z\gamma\,,$$

where xyz are scalars to be determined. Operating by $S.V\beta\gamma$ on the expression, we have

$$S\rho V\beta\gamma = xS\alpha V\beta\gamma\,.$$

But

$$V\beta\gamma = \alpha' \sin a \ (\alpha' \text{ being the vector of } A')\,,$$

therefore

$$S\rho\alpha' = xS\alpha\alpha'\,,$$

or

$$\cos PA' = x \cos AA'\,,$$

or

$$x = \frac{\sin r}{\sin p_1}.$$

Similarly

$$y = \frac{\sin r}{\sin p_2}, \quad z = \frac{\sin r}{\sin p_3}.$$

Hence

$$\rho = \sin r \left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (1.)$$

To find the vectors of the poles of the escribed circles, let $\rho_1\ \rho_2\ \rho_3$ be the vectors of $P_1\ P_2\ P_3$, the poles of the escribed circles opposite to ABC respectively. Then, as before, we may write

$$\rho_1 = x\alpha + y\beta + z\gamma.$$

Determining the scalars xyz as before, we have

$$x = \frac{\cos P_1A'}{\cos AA'}, \quad y = \frac{\cos P_1B'}{\cos BB'}, \quad z = \frac{\cos P_1C'}{\cos CC'}.$$

By geometry of the figure we see that

$$P_1A' = \frac{\pi}{2} + r_1 \quad P_1B' = \frac{\pi}{2} - r_1 \quad P_1C' = \frac{\pi}{2} - r_1.$$

Hence

$$x = -\frac{\sin r_1}{\sin p_1}, \quad y = \frac{\sin r_1}{\sin p_2}, \quad z = \frac{\sin r_1}{\sin p_3}.$$

Therefore

$$\rho_1 = \sin r_1 \left(-\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (2.)$$

Similarly we find

$$\rho_2 = \sin r_2 \left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (3.)$$

$$\rho_3 = \sin r_3 \left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} - \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (4.)$$

Coroll.:

$$\frac{\rho_1}{\sin r_1} + \frac{\rho_2}{\sin r_2} + \frac{\rho_3}{\sin r_3} = \frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} = \frac{\rho}{\sin r}$$

(a result which is useful further on).

To find vector of the pole of the circumscribed circle, let σ be the vector of Q, the pole of the circumscribed circle. Then since

any vector may be expressed in terms of any three other conterminal and not complanar vectors, we may write

$$\sigma = x\alpha' + y\beta' + z\gamma' .$$

Operate now by $S.\alpha$. Then noticing that

$$S\alpha\beta' = 0 \quad S\alpha\gamma' = 0$$

we have

$$S\alpha\sigma = xS\alpha\alpha' ,$$

or

$$\cos AQ = x \cos AA' ,$$

i.e.,

$$x = \frac{\cos R}{\sin p_1} .$$

Similarly

$$y = \frac{\cos R}{\sin p_2} , \quad z = \frac{\cos R}{\sin p_3} .$$

Hence

$$\sigma = \cos R \left(\frac{\alpha'}{\sin p_1} + \frac{\beta'}{\sin p_2} + \frac{\gamma'}{\sin p_3} \right) \quad . \quad . \quad (5.)$$

Or since

$$V\beta\gamma = \alpha' \sin a , \ \&c.,$$

we may write

$$\sigma = \frac{\cos R}{\sin a \ \sin p_1} (V\beta\gamma + V\gamma\alpha + V\alpha\beta) \quad . \quad . \quad (5.)$$

Or we might proceed thus—

Since

$$QA = QB = QC ,$$

therefore

$$S\sigma\alpha = S\sigma\beta = S\sigma\gamma ,$$

therefore

$$S\sigma(\alpha - \beta) = 0 , \ S\sigma(\beta - \gamma) = 0 ,$$

therefore

σ is $\perp^{r}$ plane of chordal Δ .

Hence

$$\sigma = zV(\beta\gamma + \gamma\alpha + \alpha\beta) .$$

Operate by $S\alpha$. Then

$$S\alpha\sigma = zS\alpha V\beta\gamma = z \sin a S\alpha\alpha' ,$$

therefore

$$z = \frac{\cos R}{\sin a \ \sin p_1} ,$$

or

$$\sigma = \frac{\cos R}{\sin a \sin p_1}(V\beta\gamma + V\gamma\alpha + V\alpha\beta) = \cos R\left(\frac{\alpha'}{\sin p_1} + \frac{\beta'}{\sin p_2} + \frac{\gamma'}{\sin p_3}\right).$$

To find the vector of the orthocentre.

Let ω be the vector of X, the orthocentre of the triangle. Then

$$\omega = x\alpha + y\beta + z\gamma.$$

To determine the scalar, operate as before by $S\alpha'$

$$S\alpha'\omega = xS\alpha\alpha'$$

or

$$\cos XA' = x \cos AA'$$

$$(\text{calling arc } XA = q_1 \quad XB = q_2 \quad XC = q_3)$$

$$\sin(p_1 - q_1) = x \sin p_1,$$

or

$$x = \frac{\sin(p_1 - q_1)}{\sin p_1}, \quad y = \frac{\sin(p_2 - q_2)}{\sin p_2}, \quad z = \frac{\sin(p_3 - q_3)}{\sin p_3}.$$

Hence

$$\omega = \frac{\sin(p_1 - q_1)}{\sin p_1}\alpha + \frac{\sin(p_2 - q_2)}{\sin p_2}\beta + \frac{\sin(p_3 - q_3)}{\sin p_3}\gamma \quad (6.)$$

Or we may proceed as follows, and express ω in terms of $\alpha' \beta' \gamma'$.
Let

$$\omega = x\alpha' + y\beta' + z\gamma'.$$

Then

$$S\alpha\omega = xS\alpha\alpha',$$

therefore

$$\cos q_1 = x \sin p_1,$$

therefore

$$x = \frac{\cos q_1}{\sin p_1}, \quad y = \frac{\cos q_2}{\sin p_2}, \quad z = \frac{\cos q_3}{\sin p_3}.$$

Hence

$$\omega = \frac{\cos q_1}{\sin p_1}\alpha' + \frac{\cos q_2}{\sin p_2}\beta' + \frac{\cos q_3}{\sin p_3}\gamma' \quad . \ . \ (6'.)$$

Having now found very simple and symmetrical expressions for the vector of these six points, we proceed to apply the results to the solution of various well-known problems.

Ex. (1.) To find the arcual distances between the poles of the circumscribed circle and the inscribed circle, also of the escribed circles.

Taking Q, P, P_1, P_2, P_3 to be these points, and σ, ρ, ρ_1, ρ_2, ρ_3 to be their vectors—

by (5) $$\sigma = \cos R\left(\frac{\alpha'}{\sin p_1} + \frac{\beta'}{\sin p_2} + \frac{\gamma'}{\sin p_3}\right),$$

by (1) $$\rho = \sin r\left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3}\right),$$

therefore

$$S\sigma\rho = \cos R \sin r\left(\frac{S\alpha'\alpha}{\sin^2 p_1} + \frac{S\beta'\beta}{\sin^2 p_2} + \frac{S\gamma'\gamma}{\sin^2 p_3}\right),$$

(noticing that $S\alpha'\beta = 0$, &c.),
therefore

$$\cos QP = \cos R \sin r\left(\frac{\sin p_1}{\sin^2 p_1} + \frac{\sin p_2}{\sin^2 p_2} + \frac{\sin p_3}{\sin^2 p_3}\right),$$

or

$$\cos QP = \cos R \sin r\left(\frac{1}{\sin p_1} + \frac{1}{\sin p_2} + \frac{1}{\sin p_3}\right).$$

Again, by (2)

$$\rho_1 = \sin r_1\left(-\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3}\right),$$

therefore

$$\cos P_1Q = -S\sigma\rho_1 = \cos R \sin r_1\left(-\frac{1}{\sin p_1} + \frac{1}{\sin p_2} + \frac{1}{\sin p_3}\right),$$

and

$$\cos P_2Q = S\sigma\rho = \cos R \sin r_2\left(\frac{1}{\sin p_1} - \frac{1}{\sin p_2} + \frac{1}{\sin p_3}\right),$$

and

$$\cos P_3Q = -S\sigma\rho_3 = \cos R \sin r_3\left(\frac{1}{\sin p_1} + \frac{1}{\sin p_2} - \frac{1}{\sin p_3}\right).$$

Adopting the usual notation, $\sin p_1 \sin a =$ &c., $= 2n$, we have (see Todhunter's Spherical Trigonometry)

$$\cos PQ = \frac{\cos R \sin r}{2n}(\sin a + \sin b + \sin c)$$

$$\cos P_1Q = \frac{\cos R \sin r_1}{2n}(-\sin a + \sin b + \sin c).$$

&c. &c.

Ex. (2.) To find the arcual distances between the orthocentre and the poles of the inscribed, escribed, and circumscribed circles.

Calling the vector of orthocentre (X) ω, we have from (6)

$$\omega = \frac{\sin(p_1 - q_1)}{\sin p_1}\alpha + \frac{\sin(p_2 - q_2)}{\sin p_2}\beta + \frac{\sin(p_3 - q_3)}{\sin p_3}\gamma,$$

therefore

$$S\omega\sigma = \cos R\left(\frac{\sin(p_1 - q_1)}{\sin^2 p_1}S\alpha\alpha' + \frac{\sin(p_2 - q_2)}{\sin^2 p_2}S\beta\beta' + \frac{\sin(p_3 - q_3)}{\sin^2 p_3}S\gamma\gamma'\right),$$

therefore

$$\cos XQ = \cos R\left(\frac{\sin(p_1 - q_1)}{\sin p_1} + \frac{\sin(p_2 - q_2)}{\sin p_2} + \frac{\sin(p_3 - q_3)}{\sin p_3}\right).$$

Again, from second form of (6)

$$\omega = \frac{\cos q_1}{\sin p_1}\alpha' + \frac{\cos q_2}{\sin p_2}\beta' + \frac{\cos q_3}{\sin p_3}\gamma',$$

therefore

$$S\omega\rho = \sin r\left(\frac{\cos q_1}{\sin^2 p_1}S\alpha\alpha' + \frac{\cos q}{\sin^2 p_2}S\beta\beta' + \frac{\cos q_3}{\sin^2 p_3}S\gamma\gamma'\right),$$

or

$$\cos XP = \sin r\left(\frac{\cos q_1}{\sin p_1} + \frac{\cos q_2}{\sin p_2} + \frac{\cos q_3}{\sin p_3}\right).$$

Similarly from (2) (6) we find

$$\cos XP_1 = \sin r_1\left(-\frac{\cos q_1}{\sin p_1} + \frac{\cos q_2}{\sin p_2} + \frac{\cos q_3}{\sin p_3}\right),$$

and similar expressions for $\cos XP_2$, $\cos XP_3$.

Ex. (3.) To find the volumes of pyramids $OP_1P_2P_3$, OP_1P_2P, &c., where O is the centre of the sphere, in terms of the volume of the pyramid OABC.

We have

$$\rho_1 = \sin r_1\left\{\frac{\gamma}{\sin p_3} - \left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2}\right)\right\}$$

$$\rho_2 = \sin r_2\left\{\frac{\gamma}{\sin p_3} + \left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2}\right)\right\},$$

therefore multiplying these, and taking the vector of each side, we have

$$V\rho_1\rho_2 = 2\sin r_1 \sin r_2 V.\frac{\gamma}{\sin p_3}\left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2}\right),$$

or

$$V\rho_1\rho_2 = 2\sin r_1 \sin r_2\left\{\frac{V\gamma\alpha}{\sin p_3 \sin p_1} - \frac{V\gamma\beta}{\sin p_3 \sin p_2}\right\}.$$

Again

$$\rho_3 = \sin r_3 \left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} - \frac{\gamma}{\sin p_3}\right),$$

therefore

$$S\rho_3 V\rho_1\rho_2 = 2 \sin r_1 \sin r_2 \sin r_3 \frac{-S\alpha V\gamma\beta + S\beta V\gamma\alpha}{\sin p_1 \sin p_2 \sin p_3},$$

or

$$S\rho_3\rho_1\rho_2 = 4 \frac{\sin r_1 \sin r_2 \sin r_3}{\sin p_1 \sin p_2 \sin p_3} S\alpha\beta\gamma .$$

Now

$$-S\rho_3\rho_1\rho_2 = 6 \text{ vol. of pyramid } OP_1P_2P_3 = 6V$$

$$-S\alpha\beta\gamma = 6 \text{ vol. of pyramid OABC} = 6V$$

therefore

$$V_4 = 4 \frac{\sin r_1 \sin r_2 \sin r_3}{\sin p_1 \sin p_2 \sin p_3} V .$$

Also

$$S\rho V\rho_1\rho_2 = 2 \sin r \sin r_1 \sin r_2 \frac{-S\alpha V\gamma\beta + S\beta V\gamma\alpha}{\sin p_1 \sin p_2 \sin p_3},$$

or

$$S\rho\rho_1\rho_2 = \frac{4 \sin r \sin r_1 \sin r_2}{\sin p_1 \sin p_2 \sin p_3} S\alpha\beta\gamma ,$$

or

$$V_1 = \frac{4 \sin r \sin r_1 \sin r_2}{\sin p_1 \sin p_2 \sin p_3} V ,$$

calling pyramid $OPP_1P_2 = V_1$, &c.

Similarly we find the vols. of pyramids OPP_2P_3, &c., and arrive at this result—

$$V_1 \sin r_3 + V_2 \sin r_1 + V_3 \sin r_2 = 3V_4 \sin r .$$

Ex. (4.) To find the radius of Dr Hart's circle, *i.e.*, the circle which touches the inscribed circle and the three escribed circles.

Let η be the vector of the pole of this circle, κ its angular radius. Then since the circle touches all four circles, we must have, if z be its centre

$$\text{arc } zP_1 = \kappa + r_1 , \quad zP_2 = \kappa + r_2 , \quad zP_3 = \kappa + r_3 , \quad zP = \kappa - r .$$

Hence

$$S\eta\rho_1 = -\cos(\kappa + r_1) = \sin\kappa \sin r_1 - \cos\kappa \cos r_1$$

$$S\eta\rho_2 = -\cos(\kappa + r_2) = \sin\kappa \sin r_2 - \cos\kappa \cos r_2$$

$$S\eta\rho_3 = -\cos(\kappa + r_3) = \sin\kappa \sin r_3 - \cos\kappa \cos r_3 ,$$

therefore

$$S\eta\left(\frac{\rho_1}{\sin r_1}+\frac{\rho_2}{\sin r_2}+\frac{\rho_3}{\sin r_3}\right)=3\sin\kappa-\cos\kappa(\cot r_1+\cot r_2+\cot r_3)\,.$$

But

$$\frac{\rho_1}{\sin r_1}+\frac{\rho_2}{\sin r_2}+\frac{\rho_3}{\sin r_3}=\frac{\rho}{\sin r}\,,$$

therefore

$$\frac{S\eta\rho}{\sin r}=3\sin\kappa-\cos\kappa\,(\cot r_1+\cot r_2+\cot r_3)\,.$$

But

$$S\eta\rho=-\cos(\kappa-r)\,,$$

therefore

$$-\frac{\cos(\kappa-r)}{\sin r}=3\sin\kappa-\cos\kappa\,(\cot r_1+\cot r_2+\cot r_3)\,,$$

therefore

$$4\sin\kappa=\cos\kappa\,(\cot r_1+\cot r_2+\cot r_3-\cot r)\,,$$

therefore

$$\tan\kappa=\frac{\cot r_1+\cot r_2+\cot r_3-\cot r}{4}=\frac{\tan R}{2}\,.$$

3. On some Remarkable Changes, Additions, and Omissions of Letters in Certain Cognate European Words. By the Hon. Lord Neaves.

The subject of comparative philology has always interested scholars, but latterly the study has been carried on in a more scientific manner, and I may also say with more success, than at any former period. One great object in prosecuting the study is to detect the various disguises which words radically the same are apt to assume in different languages or dialects. The great scholars of two centuries ago were fully alive to the importance of this inquiry, and although they sometimes indulged in too great a latitude of conjecture, there is scarcely an etymological affinity now generally admitted of which traces and indications are not plainly to be found in the works of those learned men, and more particularly in the writings of Salmasius, the greatest of them all.

But it cannot be denied that a strong impetus to this science has latterly been given, arising partly from a more extended knowledge of the forms of speech since Europeans began to study the cognate languages of the East. Comparative philology has thus assumed a more definite shape within the last fifty years, as for instance in the law of sound-change first pointed out by Rask, and afterwards confirmed and extended by Grimm.

Other phenomena of change have still more recently been made prominent, and to some of these I now wish shortly to direct attention.

An opinion prevails among several eminent philologians that the letter and sound of *l* did not originally occur in the Aryan family to which our chief European languages belong. Its introduction, if it is not original, is certainly not recent, for it would be difficult to maintain that it has not existed for several thousand years, as it plays so conspicuous a part in the Homeric writings. But it appears that the Zend language—that is, the old Persian or Bactrian—had no such letter as *l*, and that European words which have that sound have frequently Zend forms where *r* supplies the place of *l*. It is said also that in the oldest Indian writings the same peculiarity appears, though the *l* has been freely introduced into the later Sanscrit.

Be this as it may, it must be admitted that there is a great affinity between the smooth and the rough liquids, *l* and *r*, and that they are frequently interchangeable. We see much of this in Greek and Latin, and it is not easy to say that either of the two languages shows a preference for one of those letters over the other. Let us take some plain and undoubted examples:—λειριον, G., = lilium, L.; ρακος, G., = λακος, G., a ragged garment; in connection with which it has been specially observed that the Cretan form of Doric frequently confounded ρ and λ. The terminations -ρος, G., and -lus, L., seem cognate, as in τρομερος and tremulus. In Latin itself we have two terminational forms that seem identical—alis and aris—the use of which seems in a great measure determined by euphony, in this way, that where *l* occurs in the radical word, the termination -aris is used for the sake of variety; and when *r* occurs in the radical, -alis is used. Thus from *populus* comes *popularis*; and from *natura*, *naturalis*.

It is remarkable that in the later Romance languages, *l*, when it is found in Latin, sometimes disappears, and is replaced by *r*: as apostolus, apôtre; epistola, epître; capitulum, chapitre, &c. Indeed *l* does not very well stand its ground in modern times. In Italian it often becomes an *i*; in French it becomes an *u*; and in the lower German dialects, such as our own Scottish, it is similarly changed or lost.

Let us now assume as an interim hypothesis that *r* and *l* are interchangeable in Greek and Latin, and see if that assumption will afford us results that tend to confirm its truth.

The names for the *swallow* in those two languages are respectively χελιδων and hirundo. Upon the hypothesis suggested, χελιδων may be changed into χεριδων; and then, by well-known tendencies of the Latin language, the final ν will be dropped, leaving χεριδω, while an *n* may be inserted before the *d* to strengthen the syllable, as in tu*n*do, tudi; fu*n*do, fudi; fi*n*do, fidi; frago, fra*n*go; tago, ta*n*go, &c. We thus get χερινδω and hirundo, the identity of which is manifest.

Χαλαζα and grando, the words for *hail*, may be assimilated nearly in the same way. Χαλαζα becomes χαραζα: this when contracted becomes χραζα, as χαρις becomes gratia. Ζ is = to ds or di, and with an inserted *n*, χαραζα is equal to grandia, which is close upon *grando*.

Upon this footing we see the identity or near affinity of κρυπτω and καλυπτω; and with these, perhaps, κλεπτω may be connected.

Κυκλος, the Greek for a wheel or ring, may in its more primitive form be set down as κυκρος, which seems cognate to the Indian form chakra, with the same meaning. But κυκρος with a slight metathesis leads easily to the Latin circus, circulus; and it is again possible that by aspirating and modifying the consonants, circus becomes identified with the Teutonic ring = hring, while κυκλος is thought to be cognate to the Teutonic wheel; so many diversities of form may thus be derived from the same elements of a guttural twice repeated, and a liquid *r* or *l* variously arranged.

Ἑλμινθς, by changing the *l* into *r* and prefixing a digamma, becomes vermis, the relation of the aspirate and digamma being the same as in ἑσπερος and vesper. The Greek ἑδρα would be easily changed in Latin into *sedla*, which by assimilation becomes

sella. *Balbus* and *barbarus* seem in like manner to be connected, the meaning of *barbarus* being one who speaks unintelligibly.

We may here give an example of the same radical word appearing in two different forms in the same language with diversified but kindred meanings. The Greek ἀμέργω has the general meaning of pressing or squeezing, while ἀμέλγω has the special meaning of pressing out milk from the udder. The first of these has not been adopted by the other European languages, but ἀμέλγω is very widely diffused as *mulgeo* in Latin, and *milk* in the Teutonic languages.

A somewhat similar example may be found in the Greek words γραφω and γλαφω. These two words mean different methods of a kindred operation, that of marking intelligible forms by some sharp or cutting instrument, the one designating the process of writing or painting, and the other that of carving or modelling. Another cognate seems to be γλυφω. But of these words, and some others connected with them, I shall have occasion afterwards to speak more fully.

Examples of the interchange of *r* and *l* might be further multiplied, but those already given will sufficiently illustrate the subject, and direct attention to this mode of discovering latent affinities in words.

It has often been surmised that a similar relation subsists between other liquids, as between λυμφα and nympha, and it seems clear that the Attic dialect frequently changed an ν into λ, as in νιτρον, λιτρον, &c. But this hypothesis has not as yet been sufficiently matured, and I refrain from entering on it.

The next point that I shall notice is the peculiarity that undoubtedly exists, of leaving out or adding an initial sibilant in cognate words, so as partially to disguise them. In some languages combinations of letters are found to be admissible, and even frequent, which are not found in other languages, though nearly allied. Neither Greek nor Latin seems to admit of the initial combinations of *sl* or *sn*, and, accordingly, it is probable that an affinity subsists between words in other European languages which show these combinations, and Latin and Greek words that show no sibilant. Thus laxus may be a cognate of slack, limus of slime, &c. Nix, undoubtedly, is identical with snow, and

nervus is supposed to be the same as snare, which is a word for a string.

The Greek makes use of the initial combination *sm*, which the Latin rejects; but the Greek is not constant to that combination of letters, and the *s* is often dropped, so that we have σμικρος and μικρος, σμαραγδος and μαραγδος. The word μειδίαω does not show any initial σ, but it may be conjectured that the σ was once there, and has been dropped. The prosody and form of Venus's epithet φιλομμειδής would be well explained by considering it as a corruption of φίλοσμειδής. If we adopt this view, we then establish an affinity between the Greek μειδιαω and the English *smile*, which is further supported by the well-known tendency of the Greek δ to become an *l*.

In those words in other languages which have an initial *s* and a consonant, the *s* is often dropped in Latin, while the rest of the word is retained. We see examples of this in comparing Greek and Latin words. Thus we have the Greek σφαλλω becoming in Latin *fallo;* σφογγος becoming *fungus;* and σφενδόνη, *funda.* The Greek στέγω and στέγος seem to be identical with *tego* and *tectum*, and in other languages the *s* seems also to be lost, as in the Gaelic teach, the English thatch, and the German decken.

In some cases we find an initial *s* in the Teutonic languages, where it is wanting in Latin and Greek. Thus the Latin taurus and the corresponding Greek ταυρος seem to be represented by the Teutonic word steer, of which a diminutive is stirk. The Greek κειρω appears in English in the form of shear, an *s* being prefixed, and the other consonant thereby softened. This root in the Teutonic languages is very productive, there being many forms of it connected with the process of cutting or dividing, as shear, share, ploughshare, scar, score, sharp, &c. Probably, also, *short* comes from this source, and fully represents the Latin curtus with a sibilant prefixed. The Latin caveo seems to have a cognate in an Anglo-Saxon word scevian, from which come our common words of shy and shun, for the Latin caveo has completely the idea of shunning or being shy of an object. "Hunc tu Romane caveto." The Latin *carus* with careo may possibly be connected with the English *scarce*, for the radical idea in carus is that of

scarcity as adding to the value of a thing, just as the physical dearth adds to the moral dearness of an object; *parco* also in Latin may be cognate with the English spare.

The initial *l* in Latin seems often to disturb the formation of words, and to sacrifice some letter that had preceded it. Besides other examples already referred to, we may notice the word *lien*, which appears to have lost the older initials *sp*, which would identify it with *spleen*. But the most remarkable instance of this is found in the word *lis*, which from the old grammarians we know to have been originally *stlis*. But acting on the principle which identifies *l* and *r*, we see that the original form would give us *strits* instead of *stlis*, and thus we should have the word commencing with the same letters as our Teutonic words strife, sturt, streit, G., &c., which undoubtedly are cognate in their meaning with the Latin *lis*, though this word by a strange metamorphosis has lost all trace of that struggle or violent contention which it really represents.

It is a peculiarity of the Greek language that all words beginning with ρ are supposed to have a prefixed aspirate which is analogous to a Latin sibilant. If we transferred a Greek word of this kind to a Teutonic form, we should prefix a sibilant to the *r*, but as *sr* is not a combination favoured by the Teutonic languages, a *t* might be inserted for the sake of euphony. Upon this footing we may plausibly consider the Greek ῥευμα as identical with the Teutonic *stream*. It is remarkable, however, that the Gaelic has no objection to the initial combination of *sr*, and accordingly we find the word *sruth*, pronounced *srhu*, meaning a stream or current, and occupying an intermediate position between the Greek ρεω, ρευσω, and the Teutonic stream. If we could get over the change of vowel, we might in the same way connect the Greek ῥιν, a nose, with the Gaelic sron.

It seems a remarkable circumstance that in Greek and Latin words beginning with a sibilant and another consonant there is a tendency to confound or corrupt the second consonant so as to change one for another in a somewhat arbitrary way. In Latin, in particular, where the consonant succeeding the sibilant is always a tenuis, one tenuis is frequently changed for another in comparing Greek and Latin words. Thus σπουδη and studium

seem equivalent, also perhaps σταδιον and spatium, σκεπω and specio, σκυλευω and spolio.

In conclusion, on this modification of original roots by the adjection of an initial sibilant, I shall revert to a set of words already noticed—viz., γραφω, γλαφω, γλυφω. These words are of analogous meaning; and it is interesting to see whether we can find corresponding and cognate words in Latin. I think the Latin words *scalpo* and *sculpo* are in this situation. These words seem to be formed by prefixing a sibilant to the radical elements of the Greek forms which consist of a guttural γ or *c*, a liquid *l* or *r*, and a labial φ or π. Now these elements, with a slight metathesis, may become γραπ or glap, γαρπ or galp; prefixing an *s* we have scalp and sculpo, which words indicate operations of a kindred kind, that of carving or embossing. Some philologists, of whom Salmasius is one, consider that by a similar process γραφω becomes *scribo*.

I now conclude these few observations, not unconscious that some of the conjectures and speculations which have been ventured may be overstrained, or in some respects mistaken. But I feel considerable confidence that the general views I have expressed, most of which are derived from higher sources than my own opinions, are correct in substance, and are calculated to afford considerable aid in the continuous progress which philology is making.

4. De l'interpolation des fonctions irrationnelles en général, et des fonctions logarithmiques en particulier, à l'aide des tables numériques. Par F. Lefort, inspecteur général des Ponts et chaussées, membre correspondant de l'Acadèmie des Sciences de Naples.

Introduction.

Pour faire un usage intelligent des tables des fonctions irrationelles en général, et des tables de logarithmes en particulier, il est indispensable de conaître les formules fondamentales de l'interpolation, et de savoir se rendre compte du degré d'approximation que l'on peut obtenir dans les différénts cas. Je traite dans ce mémoire ces deux points qui sont à peine indiqués dans la plupart des

ouvrages élémentaires, et qui sont complètement omis dans les introductions les plus développées à des tables d'ailleurs très estimables.

De l'interpolation par le moyen des tables numériques de fonctions irrationnelles.

Une table si étendue qu'elle soit, ne peut contenir, dans les limites de l'approximation qu'elle comporte, toutes les valeurs d'une fonction, puisque cette fonction est susceptible de croître par intervalles infiniment petits, et que ses valeurs successives sont calculées pour des acroissements finis, et généralement assez bornés, de la variable. Cependant, on peut se servir de la table des valeurs inscrites pour déterminer approximativement les valeurs intermédiaires de la fonction, et c'est à la solution de ce problême que s'applique la méthode dite d'interpolation.

On démontre que toutes les fonctions qui entrent dans les tables peuvent, entre certaines limites, être développées en séries convergentes suivant les puissances entières et positives de la variable, à laquelle on donne le nom *d'argument* de la table. Si donc, dans le calcul de ces fonctions, on borne l'approximation à l'ordre n, elles pourront être assimilés à des fonctions rationelles et entieres de cet ordre. Ainsi on aura généralement

$$(1) \qquad u = a_0 + a_1 x + a_2 x^2 + \ldots . a_n x \;;$$

u étant une fonction quelconque de x, et $a_0, a, \ldots$ des quantités numériques, positives ou négatives.

Les $n + 1$ coefficients de x sont complétement déterminés, quand on connait $n + 1$ valeurs de u, répondant à $n + 1$ valeurs également connues de x. Par suite, la fonction générale de x doit être considerée comme donnée par cela seul que l'on donne $n + 1$ de ses valeurs locales.

D'un autre côté, pour la même fonction u, on a, par la formule générale des différences finies,

$$(2) \; u_n = u_0 + n\,\Delta u_0 + n\,\frac{n-1}{2}\,\Delta^2 u_0 + \ldots n\,\frac{n-1}{2}\,\frac{n-2}{3}\ldots \Delta^n u_0,$$

où n est un nombre entier tel qu'en supposant constant l'accroisse-

ment Δx de la variable, $n = \frac{x_n - x_0}{\Delta x}$; on peut donc écrire au lieu de l'équation (2)

$$(3)\quad u_n = u_0 + \frac{x_n - x_0}{\Delta x}\frac{\Delta u_0}{1} + \frac{x_n - x_0}{\Delta x}\left(\frac{x_n - x_0}{\Delta x} - 1\right)\frac{\Delta^2 u_0}{1\cdot 2} + \dots$$

Cette équation est du n^e degré en x, et elle doit devenir identique avec l'équation (1), quand on fait dans cette dernière $x = x_n$; donc les coefficients des mêmes puissances de x_n et de x sont égaux, et les deux équations ne diffèrent qu'en ce que dans l' une on emploie le symbole x et dans l'autre le symbole x_n. On peut dès lors écrire d'une manière générale.

$$(4)\quad u = u_0 + \frac{x - x_0}{\Delta x}\frac{\Delta u_0}{1} + \frac{x - x_0}{\Delta x}\left(\frac{x - x_0}{\Delta x} - 1\right)\frac{\Delta^2 u_0}{1\cdot 2} + \dots$$

formule très diffèrente de la formule (3), attendue que $\frac{x - x_0}{\Delta x}$ n'est plus assujetté à être un nombre entier, mais peut passer par toutes les valeurs comprises entre *o* et *n*.

L'équation (4) permet d'interpoler dans la série des valeurs u_0, u_1 . . . . u_n, avec le même degré d'approximation* qui a été adopté pour le calcul de ces premières valeurs. On doit remarquer d'ailleurs que, l'interpolation se faisant toujours entre deux termes consécutifs de la série, et l'origine des indices étant arbitraire, on peut toujours faire ensorte que $\frac{x - x_0}{\Delta x}$ soit moindre que l'unité. Dans ces deux conditions, et si les différences sont petites, la formule (4) devient très convergente, et on peut, dans les applications, borner le calcul à un petit nombre des termes de la série.

Par exemple, si l'on prend x_0 pour point de départ, et que l'on considère x comme exprimé en parties de Δx, on doit poser $x_0 = o$ et $\Delta x = 1$, en sorte que l'interpolation s'opère par la formule très simple

$$(5)\qquad u = u_0 + x\,\Delta u_0 + x\,\frac{x-1}{2}\,\Delta^2 u_0 + \dots$$

entièrement de même forme que l'équation (2). Toutefois il ne faut pas perdre de vue que x est une quantité plus petite que 1, une

* Nous verrons plus loin sous quelles réserves.

véritable fraction de Δx. L'usage, dans le calcul des différences finies, est de dénoter par des indices croissants les valeurs de la fonction qui répondent à des valeurs successivement croissantes de la variable. De cette manière, les différences premières sont toujours positives, lorsque la fonction croit en même temps que la variable, et négatives dans le cas contraire. Il est indispensable d'avoir ces considérations présentes à l'esprit pour ne pas commettre d'erreurs dans l'application des formules, et surtout pour ne pas leur donner une extension qu' elles ne comportent pas.

Par exemple, on ne pourrait dans la formule (2) changer u_0 en u_n, et reciproquement; mais on devrait écrire

$$(6) \qquad u_0 = u_n - n\Delta\, u_{n-1} + n\frac{n-1}{2}\,\Delta^2\, u_{n-2} + \ldots \pm \Delta^n\, u_0\,;$$

ainsi qu'il est facile de le vérifier.

Lorsque $\Delta^n\, u_0 = \Delta^n\, u_n$, on a encore

$$(7)\ u_0 = u_n - n\Delta u_n + n\frac{n+1}{2}\,\Delta^2 u_n + \ldots \pm n\,\frac{n+1}{2}\,\frac{n+2}{3}\ldots\frac{2n-1}{n}\,\Delta^n u_n.$$

Cette équation (7), comparée à l'équation (1) conduit à la suivante

$$(8) \qquad u = u_1 - x\Delta u_1 + x\frac{x+1}{2}\,\Delta^2 u_1 - \ldots$$

qui donne lieu aux mêmes remarques que l'équation (5). La formule (8) permet d'interpoler en partant de la valeur supérieure de la fonction. Ce mode d'interpolation qui est souvent avantageux, n'est pas habituellement suivi: on s'appuie en général sur la formule (5).

La problême de l'interpolation est double: il s'agit de déterminer la fonction connaissant la valeur de la variable, ou de déterminer la variable connaissant la valeur de la fonction. Dans le premier cas, il n'y a qu'à mettre en nombres la formule (5), en cherchant dans les tables les valeurs u_0, Δu_0, &c., qui répondent à l'argument immédiatement inférieur à la valeur de la variable. Dans le second cas, on met l'expression de x sous la forme

$$x = \frac{u - u_0}{\Delta u_0 + \dfrac{x-1}{2}\,\Delta^2 u_0 + \ldots}$$

et on résout l'équation par des approximations successives, en

négligeant d'abord les quantités de l'ordre $\Delta^2 u_0$ et des ordres supérieurs. La vraie valeur de l'argument répondant à la fonction u sera $x_0 + x\,\Delta u_0$.

Si l'on voulait faire usage de la formule (8), on aurait

$$x = \frac{u_1 - u}{\Delta u_1 - \frac{x+1}{2}\Delta^2 u_0 + \ldots};$$

et la vraie valeur de l'argument répondant à la fonction u serait $x_1 - x\Delta x_1$. x est toujours compté à partir de l'argument qui sert de base à l'interpolation.

Du degré d'approximation que permettent les tables usuelles.

On entend par tables usuelles celles qui ne nécessitent pas en général l'emploi des différences secondes.

Les tables usuelles les plus étendues ne donnent que les différences du premier ordre, c'est à dire les Δu. Les plus complètes présentent en outre, sous le titre de parties proportionelles, les produits de Δu par 0,1 ; 0,2 . . . jusqu'à 0,9, ou les produits de Δu par 0,01 ; 0,02 ; . . . jusqu'à 0,99. Elles ne fournissent ainsi que les deux premiers termes $u_0 + x\Delta u_0$ de la formule d'interpolation, et c'est à ces deux termes que, pour les cas ordinaires, on borne l'approximation dans la recherche de u ou de x. On écrit donc, suivant le problême à resoudre, soit

$$u = u_0 + x\Delta u_0; \text{ soit } x = \frac{u - u_0}{\Delta u_0}.$$

Il convient d'être fixé sur l'importance de l'erreur commise par suite de l'omission d'une partie des termes de la formule générale, et par suite de l'inscription incomplète des valeurs de la fonction, de ses différences, et des parties proportionelles.

Une table donnée suppose à l'avance un certain ordre d'approximation admis dans le calcul des valeurs de la fonction. Les nombres inscrits pour ces valeurs doivent être exacts à une demi-unité près de l'ordre du dernier chiffre, soit en plus, soit en moins. Ainsi les tables de logarithmes à 7 décimales doivent donner la valeur des logarithmes à une demi-unité près du 7^e ordre décimal.

D'un autre côté, les différences premières inscrites ne sont pas celles qui résulteraient du calcul direct par la formule

$$(9)\ \Delta u = f'(x)\,\Delta x + f''(x)\,\frac{\Delta x^2}{2} + \&c.,$$

en bornant l'approximation à une demi-unité du dernier ordre de la fonction; ce sont les différences mêmes des valeurs inscrites de la fonction, valeurs qui peuvent être individuellement en erreur de près d'une demi-unité du dernier ordre, de telle sorte que la différence inscrite peut être en erreur de près d'une unité de cet ordre sur la valuer complète que représente la série (9).

Enfin, les parties proportionelles, quand elles résultent des produits par les neufs premiers nombres, sont au plus données avec les dixièmes de l'unité du dernier ordre: par suite, les produits $x\Delta u_0$ peuvent être en erreur sur la vraie valeur de près d'une unité, même en supposant qu'on ne néglige aucune décimale dans la somme des produits partiels qui les composent.

Cherchons à apprécier l'importance de ces diverses causes d'erreur dans la détermination de la fonction par l'argument, ou de l'argument par la fonction.

Soit E, l'erreur propre résultant de l'omission des différences secondes et des différences des ordres supérieurs, on a évidemment

$$\mathrm{E} = x\frac{x-1}{2}\Delta^2 u_0 + x\frac{x-1}{2}\,\frac{x-2}{3}\Delta^3 u_0 + \ldots$$

Le maximum numérique du coefficient $x\dfrac{x-1}{2}$ a pour valeur 0,125, et a lieu pour $x = 0{,}5$. Le maximum numérique du coefficient $x\dfrac{x-1}{2}\,\dfrac{x-2}{3}$ a pour valeur 0,064 et a lieu pour $x = 0{,}42$. Le maximum numérique des coefficients qui suivent diminue progressivement, et répond à des valeurs progressivement moindre de x. En conséquence, si la valeur numérique des différences successives des divers ordres diminue d'une manière notable, ce qui a lieu dans les tables de logarithmes, par exemple; la série qui exprime la valeur de E, est très convergente, et il suffit en général de considérer son premier terme pour apprécier l'erreur qui resulte de l'omission des différences secondes et des différences des ordres superieurs.

Dans les tables vraiment usuelles, bien construites, le produit

$x\,\frac{x-1}{2}\,\Delta^2 u_0$, en aucun point de la table à interpoler, n'atteint une demi-unité du dernier ordre de la valeur de la fonction; par suite, l'emploi de la formule d'interpolation $u_x = u_0 + x\,\Delta u_0$ n'entraine pas une erreur d'une demi-unité pour cause d'omission des différences secondes. C'est aussi la limite d'exactitude que comportent les valeur inscrites de la fonction.

Voyons maintenant si pour l'usage de l'interpolation, en supposant toujours l'omission des différences secondes, il y a avantage à préférer les *différences tabulaires*, c'est à dire, les différences entre deux valeurs consécutives de la fonction inscrites dans la table, aux *différences vraies* que donne la formule de Taylor.

Je désigne par u, Δu les valeurs complètes de la fonction et de sa différence première, par T, ΔT les valeurs analogues inscrites dans les tables, par ΔV la différence vraie exprimée à une demi-unité près du dernier ordre de la fonction. On a

$u_x = u_0 + x\Delta u_0$. D'ailleurs $u_0 = T_0 \pm a_0$; $u_1 = T_1 \pm a_1$; a_0 et a_1 étant des quantités numériques dont la valeur est inférieure à une demi-unité du dernier ordre de la table.

On peut obtenir la valeur approximative de u_x par les formules suivantes:

$$T_x = T_0 + x\Delta T_0; \text{ ou } V_x = T_0 + x\Delta V_0.$$

Comparons entre elles les valeurs $u_x - T_x$ et $u_x - V_x$, et nous aurons ainsi l'importance de l'erreur commise dans les deux cas.

Nous remarquons d'abord que ΔV_0 et ΔT_0 ne peuvent différer que lorsque les corrections à faire à T_0 et à T_1 pour avoir u_0 et u_1 sont de signes contraires. La comparaison n'est donc à faire que lorsque

$$u_0 = T_0 \pm a_0; \text{ et } u_1 = T_1 \mp a_1;$$

les signes supérieurs étant pris ensemble et les signes inférieurs ensemble. On a alors

$\Delta u_0 = \Delta T_0 \mp (a_1 + a_0)$; et l'on peut avoir $\Delta V_0 = \Delta T_0 \mp 1$, d'où $\Delta u_0 - \Delta V_0 = \mp (a_1 + a_0) \pm 1$, on déduit de là

$$u_x - T_x = u_0 - T_0 + x\,(\Delta u_0 - \Delta T_0) = \pm\,[a_0(1-x) - xa_1] = \omega'$$

$$u_x - V_x = u_0 - T_0 + x(\Delta u_0 - \Delta V_0) = \pm\,[a_0(1-x) + x(1-a_1)] = \omega''.$$

x, $1-x$, et $1-a_1$ étant des quantités positives, la première valeur de ω'' (avec le signe $+$) est toujours positive, et la seconde (avec le signe $-$) est toujours négative. Les deux valeurs de ω' sont inversement positives ou négatives suivant que $x \lessgtr \frac{a_0}{a_0+a_1}$. Leur maximum numérique, relatif à la variable x, répond aux deux limites 0 et 1 des valeurs de cette variable.

$$\text{Pour } x = 0, \quad \omega' = \pm a_0 \quad \text{pour } x = 1, \omega' = \mp a_1,$$

ainsi la valeur numérique de ω' est toujours plus petite que 0, 5 .

Pour $x = 0$ la valeur de ω'' est la même que celle de ω', mais pour $x = 1$, $\omega'' = \pm (1 - a_1)$; la valeur numérique de ω'' pourrait ainsi l'approcher de 1.

On doit donc préférer T_x à V_x : en d'autres termes, il vaut mieux interpoler avec les différences tabulaires qu'avec les différences vraies.

Les raisonnements précédents supposent que l'on a affectué complétement le produit $x(\Delta u_0 - \Delta T_0)$ car c'est à cette condition seulement que l'on peut remplacer $\Delta u_0 - \Delta T_0$ par $\mp (a_1 + a_0)$. Cependant, en faisant usage des tables des parties proportionelles les plus complètes, telles que celles de Bremiker pour les logarithmes à 7 décimales, on ne calcule en général les produits $x\Delta T_0$ ou $x\Delta V_0$ qu'à une demi-unité du dernier ordre près. Voyons ce que deviennent alors les produits que nous avons considérés.

En général, $x\Delta T_0 = e \pm f$, e étant la partie entière du produit et $f<0{,}5$. Comme ΔV_0 peut différer de ΔT_0 de ∓ 1, on aura alors $x\Delta V_0 = x\Delta T_0 \mp x = e \pm f \mp x$.

Le seul cas à examiner est celui où la valeur numérique de $\pm f \mp x$ est plus grande que 0,5, puisque V_x ne peut différer de T_x que dans ce cas. On aurait ainsi $x\Delta V_0 = e \pm f'$, $f'>0{,}5$.

Au moyen des équations ci-dessus la valeur complète de u_x peut prendre les formes suivantes :

$$u_x = u_0 + x\Delta u_0 = T_0 \pm a_0 + x\Delta T_0 \mp x(a_1 + a_0) = T_0 + e + \omega' \pm f\,.$$

Si l'on prend pour valeur de u_x, $T_x = T_0 + e$, l'erreur réelle $\omega_r = \omega' \pm f$. Elle est numériquement plus petite que 1.

Si l'on prend au contraire pour u_x, $V_x = T_0 + e \mp x$; l'erreur

réelle est exprimée par $\omega'_r = \omega' \pm f \mp x$, et elle peut devenir numériquement plus grande que 1.

Donc, en interpolant avec les différences tabulaires, et réduisant les parties proportionelles à leurs parties entières, on est sur de ne pas commettre une erreur qui s'élève à une unité. L'erreur, au contraire, pourrait être supérieure à une unité, si l'on interpolait dans les mêmes conditions avec les différences vraies réduites à leur partie entière. Telle est la raison qui doit faire préférer, au point de vue des approximations, les différences tabulaires aux différences vraies.* Il y en a d'autres, d'ailleurs, quand on envisage la question sous la rapport de la facilité des inscriptions et des vérifications.

Il est à peine utile de dire que la différence tabulaire à adopter est celle qui existe réellement entre les deux nombres consécutifs qu'il s'agir d'interpoler, et non une différence plus ou moins voisine inscrite en marge de la table.

Pour obtenir avec sûreté la partie entière de $x\Delta T_0$ à une demi-unité près, x étant généralement un nombre de deux chiffres au moins, il faut que la table des parties proportionelles donne les dixièmes, si elle fournit seulement le produit de ΔT_0 par les neuf caractères de la numeration décimale.

Toutes les erreurs que nous venons d'apprécier s'appliquent à la détermination de la fonction à l'aide des valeurs données de l'argument. Il importe aussi de se rendre compte de la manière dont les erreurs qui peuvent entacher l'expression de la fonction et de ses différences pèsent sur la détermination de l'argument. On y parvient, sans entrer dans de longs details de calcul, en remarquant que, pour des amplitudes locales et restreintes, les variations des arguments sont à très peu près proportionelles aux variations des fonctions. Si donc la variation Δu de la fonction répond à la variation Δx de l'argument, pour une variation a de la fonction l'argument variera de $\frac{a}{\Delta u}\Delta x$. Cette variation sera d'autant plus faible que la différence de la fonction sera plus grande.

La même considération sert à apprécier l'étendue de l'erreur,

* Il est facile de conclure de ce qui precède que l'on doit également préférer les différences tabulaires pour les ordres supérieures, lorsqu'il est nécessaire d'en faire usage.

lorsque l'argument qui sert à déterminer la fonction n'est lui-même qu'approximativement connu. Si β représente l'erreur possible sur l'argument, la plus grande erreur, dont puisse être entachée pour cette cause la fonction qu'il détermine, est exprimée par $\frac{\beta}{\Delta x}\Delta u$, et l'on voit qu'inversement à ce qui avait lieu dans le cas précédent, l'erreur est d'autant plus considérable que la différence de la fonction a une plus grande valeur.

Lorsque l'on veut apprécier l'influence totale possible de diverses causes d'erreur sur la détermination d'une quantité, il faut donner le même signe aux erreurs possibles calculées, et les ajouter.

Si l'on ajoute entre elles plusieurs quantités qui ne sont exactement connues qu'entre certaines limites, la plus grande erreur possible de la somme sera égale à la somme arithmétique des plus grandes erreurs de chacun des termes, ensorte que l'expression de cette erreur ne change pas quand il s'agit de différence au lieu d'addition.

Dans la multiplication ou dans la division d'une quantité qui n'est pas exactement connue la plus grand erreur croît ou diminue dans la même proportion que la quantité elle même.

Les principes exposés dans cet article permettent de se rendre compte de l'avantage que peut présenter, suivant les cas, l'emploi de l'une ou de l'autre des formules

$$u_x = u_0 + x\Delta u_0 ; \; u_x = u_1 - x\Delta u_1 .$$

x devient ainsi égal à 0,5 au plus, et on atténue l'erreur possible sur le produit $x\Delta T_0$. Si l'on consentait à employer concurrement ces deux formules, dans les conditions que nous avons définies, on pourrait diminuer de près de moitié l'étendue des tables des parties proportionelles. On ne doit pas se dissimuler toutefois que ce double usage exige la coup d'œil d'un calculateur exercé.

(Extrait d'un mémoire sur la théorie des logarithmes, la construction et l'usage des tables logarithmiques, composé en 1857 et resté inédit.

C'est de ce mémoire qu'à été également extrait l'article sur les grandes tables du Cadastre, publié en 1858, dans le tome iv. des Annales de l'Observatoire de Paris.)

Monday, 5th July 1875.

SIR ROBERT CHRISTISON, BART., Hon. Vice-President, in the Chair.

The following Communications were read:—

1. The Theory of the Causes by which Storms Progress in an Easterly Direction over the British Isles, and why the Barometer does not always indicate real vertical pressure. By Robert Tennent, Esq.

Upwards of three years ago the author laid a paper before two members of the Scottish Meteorological Society. The question taken up was, why horizontal movement takes off vertical pressure; and the conclusion arrived at was, that every such horizontal current, owing to its passage over a resisting surface, and by means of rapid upper currents, caused removal of air and lifting, and thereby diminished pressure. It was inferred that the barometer which represented this was consequently an "effect" and not a cause of wind.

The present remarks will be confined mainly to the mechanical effects of motion and friction,—the important questions of temperature, vapour, rotation, external high and low pressure, &c., not being now considered.

Friction.—This forms a very important element. To it is due the retardation of the surface currents, while the upper currents move more rapidly, being comparatively free and unimpeded. Surface retardation is increased by pressure, which amounts to 8½ tons on every square yard, but this gradually diminishes upwards. Tyndal, by experiment, estimated the mobility of the uppers on Mont Blanc as being twice as great as that of the surface. When the atmosphere is in a state of rest, its columns may be represented as being vertical or upright, but when rapid uppers and retarded surface currents prevail, it may then be regarded as moving in inclined columns at an angle to the surface, and in the direction of the moving force.

Supply.—The inclination of the columns will depend not only on surface friction, but also on the supply of air to the moving cur-

rents. This may be sufficient, insufficient, or more than sufficient. The supply to the uppers may differ in amount from that to the surface currents. The position and the distance of the source of supply are also important. This may be vertically or horizontally situated. According to Redfield and others, the horizontal extent of an atmospheric disturbance is often two hundred times greater than its vertical height. The arresting effect of such an extensive surface on the supply drawn over it must be great. But if supply is derived from a vertical source, as in the case of a descending current, much less retardation will take place. Hence when the supply to the surface current is from a horizontal source, great inclination of columns will take place, but when from a vertical source, there will be less inclination. An important difference in the mode of inflow of the different winds will thus exist betwixt those vertically and those horizontally supplied. The former will move freely in nearly upright columns, the latter in columns more or less inclined.

Gradients.—A river flows on an incline, by the amount of which its velocity and volume are regulated; but the river itself exerts a reactionary influence on this incline, which it will tend to pull down and lower, if it is not composed of rigid materials, An aerial gradient is not rigid, it is elastic and mobile, and being thus subject to the reactionary influence of the air which it draws to itself, it will not remain stationary, nor will its incline remain unaltered. Its efficiency and the amount of its slope will therefore depend on the amount of facility with which air inflows to it. If the inflow takes place in vertical columns, little or no reactionary influence or lowering will be produced; but if the inflow is in inclined columns, which therefore produce difficult supply, being from a horizontal source, the tendency will be to pull down and lower the gradient, and thereby remove the source of supply to a greater distance. What thus takes place is popularly expressed by the phrase, that the wind blows itself out, which is in fact accomplished by lowering the gradient, and removing the source of supply to such a distance that it is almost entirely arrested by the extent of the resisting surface over which it is now compelled to pass. Thus a gradient represents not only a motive force, but also a reactionary force which is due to it.

Curve of Outward Propagation.—There are thus two different

modes of inflow towards the low central barometer; one is an advantageous, the other is a disadvantageous form. It is by this latter mode that the gradient is lowered. It takes place with inclined columns, resulting from rapid uppers and retarded surface currents. Much of the work of inflow is thus thrown upon the uppers. To enable them to maintain their superior velocity, they themselves must be adequately supplied by the uppers in advance. This is accomplished by outward extension; they advance forwards to procure the requisite supply from the still atmosphere a-head, which now begins to inflow spirally. It is to this advancing line of removal, that the term "Curve of Outward Propagation" is applied. It may be illustrated thus:—If a river flowing down an incline does so uniformly, and at an equal rate of speed, removal will equal restoration; but if in the lower part of its course, a more rapid removal is inaugurated, while restoration or supply above remains as before, the curve representing the poin at which the increased removal begins to travel upwards will represent the forward movement of this curve of outward propagation or extension.

When a rapid fall of the barometer takes place, if the inflow to the depression so formed assumes an advantageous form, it will fill up at once; but if, as usually takes place, it assumes the disadvantageous form or mode of inflow, instead of filling up, it will open out and extend itself outwards all round, like the undulations produced by a stone thrown into still water. The uniformity of this extension will depend on the uniformity of the motive central inflow. With inflowing winds of different degrees of density, temperature, and moisture, it may be safely asserted that such uniformity of inflow will not occur. A disadvantageous mode of inflow will consequently take place in one segment, with a less disadvantageous mode in another. The first is found in the advancing segment, the latter in the rear.

The effect of this want of uniformity in the mode of inflow will be, that the depression, instead of extending itself uniformly all round, *will shallow itself out in one particular direction*, which is that in which the disadvantageous mode of inflow is found, and where the curve of outward propagation exists. This disadvantageous mode of inflow is increased by the circumstance, that as the

uppers are rapid, while the surface winds are retarded, the numerous intermediate layers betwixt these, must all move at different rates of speed attended by much friction and consequent retardation.

The direction assumed by the curve, is therefore one which is *nearly opposite to that of the motive inflow, which produces it.* In a somewhat similar manner within the tropics, oceanic currents are in certain cases produced, moving in nearly an opposite direction to that of the North East trades, on which they depend.

Winds representing the different Modes of Inflow.—On the west segment of a barometric depression, Polar winds prevail, which are dry, cold, and dense, and are fed by descending currents, with a vertical source of supply. They may be regarded as surface winds. On the east segment are found equatorial winds, which are warm, moist, and less dense; they are weakened by their ascending tendency,—they have not so much the character of surface winds, but assume more the character of rapid uppers, and instead of a vertical, have a horizontal source of supply.

Progress.—When a barometic depression is formed, a spiral inflow towards the centre takes place; if this were equally uniform in every direction, the great central fall of the barometer would extend itself all round, gradually diminishing as it proceeds outwards towards the circumference, and lowering the surrounding gradients as it proceeds; but if, as is usually the case, the inflow is not uniform, the depression will then extend itself in one particular direction, in the manner above described. This extension, which is due to the mode of the central inflow, takes place mostly in front, and in an easterly direction: it will there create a scarcity of supply, towards which the low central barometer will advance. What thus takes place may be illustrated in this manner:—Suppose that the ascent of a balloon, situated near the surface of the ground, is retarded, though not arrested, by a chain passing over it. This chain, where it reaches the ground on each side, is not fixed to it, but is laid outwards along its surface, one end extends for a short distance, the other for a considerable distance. Under these circumstances, the ascent of the balloon will not be vertical, but in a direction inclined towards that in which the chain extends for the greatest length over the ground,

and where, consequently, it is most difficult to lift, and where the drag is greatest.

In a somewhat similar manner the low central barometer, having by means of the peculiar mode of inflow of winds in that segment caused supply there to be scarce, *will itself move in that direction*, to obtain the requisite supply, which it could not procure if it remained stationary: in so doing, it opens out the depression in front, and is enabled to move forward, provided it is sufficiently supplied in the rear. If high pressure or steep gradients existed in front of one of the segments, progress could not there take place, since supply being there abundant, no lowering of the gradient could take place, nor could it shallow itself out in that direction.

Lifting.—From the greater mobility of the atmosphere in the upper regions, it there moves faster, and hence the air is more easily removed than it is near the surface. The atmosphere may thus be conceived to be divided into a number of spherical concentric layers, each possessing a different rate of speed, slipping or sliding over those underneath with an increasing amount of friction, as their position becomes lower. The upper layers possess two sources of supply—one from a horizontal source, the other from the layers underneath, while the surface layers possess only a horizontal source of supply. The facility with which the uppers are thus supplied, tends in the first instance to increase their speed, but when this has taken place to a certain extent, the source of supply will diminish in amount. This is accompanied by a lowering of the gradient, the effect of which is to remove the source of supply to a greater distance, and increase the diminution, until a point is at last reached in which it is almost entirely arrested. When this begins to take place, the uppers will tend to lift and become detached as it were from the surface, thus causing a partial vacuum near the surface.

Lifting may be illustrated by what takes place on the lee side of a house or wall, over which a strong wind blows, a partial vacuum is here formed. The friction which retards the air when flowing over an extensive horizontal surface, may be represented by a series of such obstructions which enable the air to be more easily carried off and removed than it can be restored. This removal

causes a local or partial reduction of pressure, while the real vertical pressure of the atmosphere overhead remains unaltered. The relation which exists betwixt pressure and the speed of the winds is altered as their velocity increases, in a somewhat similar way to that which takes place when the lee way of a ship is practically diminished by an increase in its head way.

It must be observed, that lifting can only take place where scarcity of supply exists. The vacuum formed behind a wall over which the wind blows is due to the fact, that removal is there greater than restoration; for if supply was sufficient no such vacuum could exist.

Water flowing from an orifice in the side of a cistern, which is only a little below its surface level, will fall directly downwards; but if the level is raised much above that of the orifice, the water passing through it will be expelled with considerable force; it will "lift" and take a form approaching to that of a horizontally flowing spout. The great mass of water will accumulate in the upper part of the curve of the spout, and will connect itself with the side of the cistern by a thin film of water, which will now take the place of the large body of water which fell vertically downwards when the pressure was less. This accumulation may be taken to illustrate what takes place in the upper part of the atmosphere, while the thin film may represent the diminished pressure at the surface.

Since scarcity of supply exists in the advancing portion of a progressing depression, it is there that lifting is most highly developed. Copiousness of supply is found in the rear, and hence it is there that lifting is least likely to be found.

Lifting takes place where inequality exists in the movement of the various atmospheric layers, hence for this, among other reasons, mountain heights cannot be measured during the prevalence of strong winds, nor is the reduction of the barometer from considerable heights to sea-level at all to be depended upon. Lifting is always preceded by removal of air; but so far as removal alone is concerned, it is accurately represented by the barometer. The *diminished* pressure at the surface due to lifting is also correctly exhibited by the barometer there placed, but in such cases the barometer fails to exhibit the real vertical pressure due to the

mass of the column existing overhead. To ascertain this accurately, observations would require to be taken by a series of barometers placed at different heights, and not very far apart.

This tendency of air to accumulate aloft with the abnormal pressure which accompanies it, will be masked by the greater proportional removal of air which takes place at an upper, as compared with a lower station. This is seen in the observations at Geneva and St Bernard. The relations betwixt the pressure which exists at an upper and a lower station will thus be altered in two ways; first, by the tendency of the air to accumulate aloft, which lowers the surface barometer, while it tends to raise the upper barometer; and secondly, *by the greater proportional removal of air* which takes place aloft, depending on the height of the upper station. For this reason, the surface barometer, although it falls with strong winds, will not fall to the same extent as the upper barometer, where so much removal takes place.

Isobarics.—Lifting takes place in front of an advancing depression where supply is scarce; the pressure there indicated is consequently less than it ought to be. In the rear, where supply is more abundant, and where lifting to the same extent does not take place, the barometer there will more nearly indicate real pressure* than it does in front. Hence an isobar in front is not comparable with the same isobar in the rear. An isobar therefore would require to be corrected all round, but in different degrees; when corrected, it would extend further forward, and be more widened out in the advancing segment where progress takes place. Until such a correction is carried out, no uniformity of inflow, either in point of force or of direction can be expected from the present mode of construction of charts. Instead of isobars, this might be represented by a line or curve of Isorhoics, drawn to represent lines of equal inflow. Such an Isorhoic Curve would neither coincide with lines of equal observed pressure, nor with lines of real pressure.

The Weather Charts are, as at present constructed, drawn from

* In the subsequent use of the term "real pressure," the meaning to be conveyed is this.—The real amount of pressure due to the height of the atmospheric column overhead, but which may not be correctly indicated by the surface barometer.

observed pressure, and they are also supposed to indicate *real* pressure. But since the Isobars in front are more under the influence of the dynamical element than those in the rear, real pressure is there represented as being lower than it ought to be.

If the difficulties attendant upon the construction of a chart of Isorhoics could be overcome, it would exhibit a practical standard of reference as to the real amount of inflow of air, which cannot be ascertained by the present system of Isobarics. The introduction of the dynamical element complicates the forms of the Isobaric curves to such an extent as often to render them absolutely uninterpretable: this is done by creating barometric oscillations, and different modes of inflow in the various winds, which would not take place on a frictionless surface.

Barometer, how it represents Pressure.—It is only when the atmosphere is in a state of perfect rest that the surface barometer exhibits the real amount of pressure due to the column of air overhead, and it is only then that the normal diminution of pressure due to the diminished mass takes place in ascending upwards.

But when the atmosphere is in a state of motion and the upper currents move rapidly, the dynamical element then enters more largely into these, than into the slower moving surface currents. The consequence is, that the surface barometer will no longer indicate real pressure.

Owing to the lowering of the gradient in front, this diminution of surface pressure takes place, most in front of a moving depression, and least in the rear. It is due to lifting, hence the barometer, to a certain extent, represents dynamical or fictitious pressure. In the rear it more nearly indicates static or real pressure. No difference of *real* pressure, therefore, seems necessarily to exist here, setting aside, in the meantime, the effects of condensation, to which the reduction of pressure in the advancing segment is usually attributed. Hence it is to the difference betwixt static and dynamic pressure that progress is due.

When a gradient is lowered by friction, the accompanying lowered barometer is an effect, and in so far as it is so, it is incapable of attracting air. The gradient thus lowered is caused to extend itself forwards, and its accompanying barometer will con-

sequently fall at places which it *would not otherwise have reached*; it will there exhibit fictitious pressure, and what takes place may be explained by the use of the term "falling for sympathy with adjoining barometers."

A very considerable portion of the numerous barometric oscillations, which so constantly take place, are an *effect* due to friction from a resisting surface. On a frictionless surface their amount would be greatly reduced; no difference would then exist betwixt the speed of surface and upper currents; and depressions, with their accompanying disturbances, would then no longer possess a self-moving power,—they would cease to move forward, except, perhaps, in imbedding currents.

Conclusions.—When a barometric depression is formed, the winds inflow spirally towards the centre, but they very seldom do so equally or uniformly. In front they do so with difficulty, owing to the peculiar mode of inflow which there takes place; in the rear, they do so with comparative facility. In these circumstances, the low central barometer cannot remain stationary. It will move forward in that direction in which supply is most scarce, and by so doing, it will be enabled to procure the necessary amount of supply, which it could not have received if it remained stationary. It is in this way that progress takes place. A depression thus possesses within itself a self-moving power. When a barometer begins to fall rapidly, the fall may extend itself uniformly all round over the surrounding area. Such uniformity of extension, however, does not usually take place, except when a depression remains stationary. It generally assumes *some particular direction*, which is that indicated by progress. This is due to the difference betwixt dynamical and statical pressure in front and rear, or, perhaps more frequently, to the difference betwixt the amount of dynamical pressure to be found in these positions.

When the atmosphere is in a state of perfect rest, the barometer then indicates real vertical pressure; but when it is in motion, and the surface currents are retarded by friction, while the dynamical element of motion is introduced in a comparatively larger proportion into the uppers, the process of lifting takes place, by which surface pressure, as indicated by a barometer there placed, is reduced, while the real vertical pressure of the column of air aloft

remains unchanged. Hence, in these circumstances, a barometer does not exhibit real vertical pressure.

When horizontal movement takes off vertical pressure, this is accomplished in two ways,—first, by actual removal of air, and secondly, by lifting. Strictly speaking, however, no real horizontal movement takes place.

Addendum.

Isobarics.—As above stated, these do not indicate the existence of real pressure. There are three modes in which a barometric chart may be constructed. It may be made to exhibit real pressure, it may exhibit dynamical pressure, or it may assume the form of isorhoic curves, which will represent the correction of barometric pressure.

1. Charts of Real Pressure.—These are the daily weather charts, and the curves are there drawn through figures of equal observed pressure, but they do not exhibit the effects of the introduction of the dynamical element, and hence do not represent real pressure as they are supposed to do. To enable them to do so graphically, the curves in front must be widened out and extended forwards to such a point that the amount of pressure which they there indicate will correspond with an equal amount of observed pressure in the same isobar in the rear. Let the observed pressure in the rear of the isobar in question amount to 30·00, and let it also be supposed to be real, the front of it on the chart will be exhibited as 30·00. Let the dynamical lowering there, however, amount to *say* 0·20. An isobar, therefore, drawn through an observed and real pressure of 30·00 in the rear, will in front require to be extended forwards and drawn through an observed pressure of 30·20, to make it exhibit one which is real and comparable with that in the rear. All its parts will now be graphically comparable. The comparative wideness of the isobars in front, with the corresponding diminution in the steepness of the gradients there, will thus represent a greater scarcity of supply there than is to be found on charts of the usual construction.

A curve thus drawn, although its different portions are thus rendered graphically comparable, will not be one of isobarics, as it is not drawn through figures of equal observed pressure, nor will it

indicate the spots at which real pressure actually exists. This may illustrate the result of the present mode of construction in which the existence of real pressure is assumed.

2. Charts of Dynamical Pressure.—If barometric charts, instead of exhibiting real, are supposed to indicate dynamical pressure, this also can be exhibited graphically, and such portions of its curve also be made comparable by using figures of equal observed pressure, corrected, however, to represent those of equal dynamical pressure through which the curve will be drawn. As in the former case, such a chart will not be one of isobarics; it will be one of isodynamics, and will indicate approximately the spot at which real pressure exists.

Such a chart, therefore, will more nearly represent the real state of pressure than one of the ordinary construction, because in few or no instances, over the area usually embraced by it, is the atmosphere in a state of perfect rest, hence real pressure is not often found to exist. In this instance of graphical delineation to exhibit dynamical pressure, an extension of the isobars will take place, but to a greater extent in front than in the rear, though the extension will not be so great as in the former instance.

3. Curve of Isorhoics.—Such a curve as this, which represents uniformity of inflow in the various segments, besides aiding the correction of the barometer, will also increase the reliability of the gradients on which they depend.

Lifting.—It is generally assumed that the force of the wind depends on the steepness of the gradients, and not on the absolute height of the barometer. In the "English Meteorological Magazine," for June 1869, Strachan, however, shows that strong winds generally are also accompanied by a remarkable reduction of pressure. This takes place mostly with equatorial winds, attended also, as shown in a diagram, with the greatest barometric oscillations. The reason of this is, that these winds are fed from a horizontal source of supply, and are drawn over a resisting surface often of great inequality. Under these circumstances "lifting" takes place. With polar winds, which have a vertical source of supply, removed from proximity to a resisting surface, fewer oscillations take place, and the barometer often rises. Jenyns has shown that, unlike the thermometer, the barometer rarely rises

above its mean more than one-half of the amount to which it falls below it. When below the mean, equatorial winds prevail, and the greater range of pressure which then takes place I attribute to the cause above stated.

Note.—In the "Philosophical Magazine" for September 1874, Mr Tylor comes to the conclusion that "the barometer cannot give a true indication of weight when there is motion in the atmosphere."

2. On Electric Images. Professor Tait.

3. Laboratory Notes. By Professor Tait.

a. On the Origin of Atmospheric Electricity.

This was a preliminary notice of the results of a series of experiments devised to test the part played by water-vapour in the production of atmospheric electricity. While water is in the form of vapour it must be electrified by contact with the gases of the atmosphere—as they are by contact with one another. Precipitation of vapour in a receiver, whether produced by cold or by exhaustion, was found to be steadily accompanied with a disengagement of electricity. Further experiments are to be made with receivers of very great capacity.

b. Preliminary Experiments on the Thermal Conductivity of some Dielectrics. By Messrs C. M. Smith and C. G. Knott.

These experiments were suggested by observations on the different lengths of time required, under different circumstances, for telegraph cables to assume the temperature of the water in which they were submerged. The method employed was that known as "Ångström's," which has already been described by Prof. Tait (Proc. R. S. E. 1872–73 p. 55–61); the manner of the application of the method being, however, somewhat modified in these experiments, we will give a short description of it.

The substances to be experimented on were obtained in sheets, from which eight or nine circular discs 3 inches in diameter were cut, and piled one on another so as to form a cylinder. Between the first four discs thermo-electric junctions of fine copper and iron wires were inserted. A $\frac{1}{8}$-in. copper plate, with three small hooks on the circumference, was placed on either end, and the whole, having been slightly compressed in a Bramah press, was tied together with strings stretching from hook to hook. The cylinder thus prepared was surrounded with cotton wool, and placed horizontally in a wooden frame, with one end projecting about a quarter of an inch. Over this end a sheet of tinned iron was then drawn so as to screen all except the copper plate from the heat. The hitherto free ends of the thin copper and iron wires were attached to similar pieces of thick copper wire, and to insure equality of temperature were immersed in small vessels of water, placed in a larger vessel also containing water; the other ends of the thicker wires were then carried to the mercury pools of a commutator, so arranged that the junctions could be thrown singly, and in rapid succession, into the circuit of a Thomson's dead beat mirror galvanometer of about 24 ohms resistance. A further resistance of about 30 ohms was also placed in the circuit. The source of heat was a large vessel of boiling water. From one side of this vessel, which was placed on a movable retort stand, a cylinder, with a flat end $3\frac{1}{2}$ inches in diameter, projected for about an inch and a half.

The method of observation was as follows. The water being kept boiling, the vessel was applied for ten minutes with its flat surface *in contact* with the copper plate, then removed to a distance for ten minutes, then again applied for ten minutes, and so on during the whole of the experiment. After this had been continued for about two hours observations were begun. The galvanometer deflections for each of the three junctions were read every minute, the readings being taken from the coldest to the hottest; 15^s were taken to read the three. These readings were continued till two or three complete periods had been observed after the steady periodic state had been arrived at. The deflections thus obtained were plotted in terms of the time; and from the curves so obtained the necessary calculations were made.

Making use of Fourier's theorem, in the form,—

$$y = A_0 + A_1 \cos \frac{2\pi}{T}t + A_2 \cos 2\frac{2\pi}{T}t \; . \; . \; . \; . \; . \; . \; .$$

$$+ B_1 \sin \frac{2\pi}{T}t + B_2 \sin 2\frac{2\pi}{T}t \; . \; . \; . \; . \; . \; . \; .$$

values of A and B were obtained from the expressions

$$4\sqrt{2}A_1 = y_0\sqrt{2} + y_1 - y_3 - y_4\sqrt{2} - y_5 + y_7$$
$$4\sqrt{2}B_1 = y_1 + y_2\sqrt{2} + y_3 - y_5 - y_6\sqrt{2} - y_7 \, ,$$

where y_0, y_1, &c., are the measured values of the ordinates of the curve, taken at intervals of $\frac{1}{8}$ of a period, the axis of t being a tangent to two of the vertices, or a line parallel to it. From these values of A_1 and B_1, a and β were calculated, so as to fulfil the conditions

$$a = \sqrt{A_1^2 + B_1^2} \; ; \; \beta = \tan^{-1}\left(-\frac{B_1}{A_1}\right).$$

These having been calculated for the curves representing the oscillatory state of temperature at two of the junctions, the value of the conductivity (K) was obtained from the equation

$$\frac{K}{cp} = \frac{\pi}{T} \cdot \frac{\tau^2}{\log_e \frac{a}{a'} \times (\beta - \beta')},$$

where cp is the water equivalent, T the periodic time, and τ the distance between the two junctions. Care must be taken that $\log \frac{a}{a'}$ is the Naperian logarithm, and that $\beta - \beta'$ is measured in radians.* The unit employed in measuring y is of no importance, but τ and T must be measured in the units in which the answer is required. In the following calculations the units employed are millimetres and seconds.

The substances experimented on in these preliminary investigations were, Siemens' gutta-percha, the same as is used by Messrs Siemens Brothers in the manufacture of their cable core, and Hooper's india-rubber, which is the insulating material used by the

* The unit angle has been named by Prof. J. Thomson a *Radian*. As there is no surface conduction in these experiments, the two quantities referred to ought to be equal. Their more or less close agreement may be taken as a test of the accuracy of each experiment.

"Hooper" Company. The following are the most important results of the experiments:—

Gutta-Percha, No. I.

$\tau =$ ·99 mm.; mean free temperature, 40° C.

Junct.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	α	β
I.	0·0	4·8	11·2	18·6	24·4	20·3	12·9	5·6	− 11·13	− 0·86	11·17	175° 34′
II.	1·7	1·8	8·0	13·4	18·9	18·7	11·7	6·7	− 8·46	− 2·72	8·89	162° 10′

$$\log_e \frac{\alpha}{\alpha'} = 0·22835;\ \beta - \beta'^{*} = 13° \ 24' = 0·2345 \text{ radians};$$

$$\frac{K}{c\rho} = 0·0479 \text{ mm. secs.}$$

Gutta-Percha, No. II.

$\tau =$ 2·04 mm.; mean free temperature, 46° C.

Junctions.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	α	β
I.	2·0	6·9	12·7	18·0	17·2	11·9	6·7	1·7	− 7·59	3·48	8·35	24° 45′
II.	0·0	1·6	5·9	11·1	13·0	9·8	5·6	1·6	− 6·37	0·31	6·38	2° 47′

$$\log_e \frac{\alpha}{\alpha'} = 0·26915;\ \beta - \beta' = 21° \ 58' = 0·38397 \text{ radians};$$

$$\frac{K}{c\rho} = 0·0494 \text{ mm. secs.}$$

The third junctions in the gutta-percha did not give reliable results, owing to the very slight temperature variations.

India-Rubber, No. I.

$\tau =$ 3·320; mean free temperature, 23° C.

Junctions.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	α	β
I.	2·1	5·4	9·2	11·9	9·7	6·1	2·1	0·3	− 4·08	3·69	5·50	42° 10′
II.	0·1	1·6	4·9	7·8	8·4	6·6	3·7	1·1	− 4·13	0·60	4·17	8° 16′

$$\log_e \frac{\alpha}{\alpha'} = 0·27687;\ \beta - \beta' = 38° \ 54' = 0·5923 \text{ radians};$$

$$\frac{K}{c\rho} = 0·176.$$

* These values of $\beta - \beta'$ include ·0006 radians, being the equivalent for the 7·5″ lost in reading.

Curve III. is in this experiment not so good as curves I. and II., but the mean result from it makes $\frac{K}{cp} = \cdot 257$ with a mean free temperature of 20 C.

India-Rubber, No. II.

$\tau = 2{\cdot}480$; mean free temperature, $\left\{ \begin{matrix} \text{I.} \\ \text{II.} \end{matrix} \right\} 30^\circ$ C; $\left\{ \begin{matrix} \text{II.} \\ \text{III.} \end{matrix} \right\}$ ° C.

Junctions.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	a	β
I.	9·4	13·7	15·6	13·0	8·2	3·2	0·4	4·7	0·69	7·12	7·15	95° 32′
II.	3·8	7·7	11·2	12·0	9·0	5·1	1·4	0·9	– 2·80	4·87	5·62	60° 06′
III.	0·5	2·7	6·7	9·8	9·4	6·5	3·0	0·8	– 4·49	1·84	4 85	22° 17′

$$\left. \begin{matrix} \text{I.} \\ \text{II.} \end{matrix} \right\} \log_e \frac{a}{a'} = 0{\cdot}24078 ; \ \beta - \beta' = 35^\circ\, 26' = 0{\cdot}6190 \text{ radians;}$$

$$\frac{K}{cp} = 0{\cdot}1080 .$$

$$\left. \begin{matrix} \text{II.} \\ \text{III.} \end{matrix} \right\} \log_e \frac{a}{a'} = 0{\cdot}14739 ; \ \beta - \beta' = 37^\circ\, 49' = 0{\cdot}6607 \text{ radians.}$$

$$\frac{K}{cp} = 0{\cdot}1653 .$$

These experiments seem to show, in the case of the india-rubber, a very marked *increase* in the value of $\frac{K}{cp}$ with a decrease of temperature; but, unfortunately, the late period of the session at which the specimens were obtained prevented our repeating the experiments, which probably give too high values. We have not yet been able to obtain values for c, but hope to do so at some future time. The values for ρ are roughly—for gutta-percha, $\rho = 0{\cdot}97$; for india-rubber, $\rho = 1{\cdot}17$ at the temperature of 18·8° C.

In conclusion, our thanks are due to Prof. Tait for the use of his laboratory, and the kind assistance which he gave us in our experiments; to Prof. Jenkin, through whom we obtained the specimens; and to Messrs Siemens and Hooper for the care with which these specimens were prepared.

4. A Chapter on the Tides. By the Rev. James Pearson, M.A., Vicar of Fleetwood. Communicated by Professor Tait.

5. Farther Researches in very perfect Vacua. By Professors Dewar and Tait.

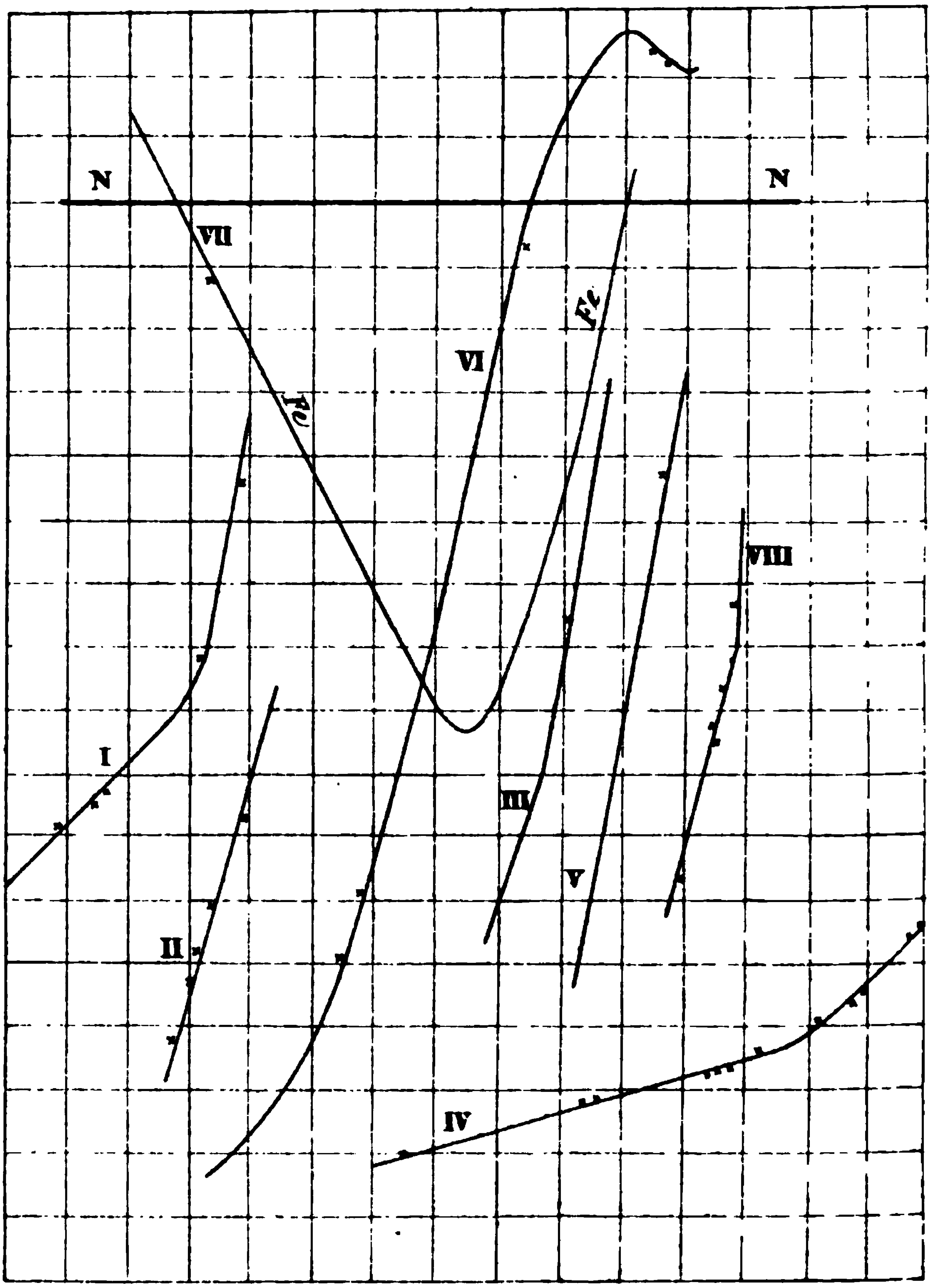
(Abstract.)

The paper commences with an account of various methods of producing very perfect exhaustion of a receiver, especially that recently devised by the authors, in which the absorbent power of charcoal is made use of. An attempt is made to calculate the amount of exhaustion thus producible.

Certain experiments described long ago by Bennett, Mark Watt, and others, and very recently much extended and improved by Crookes, are next referred to; with the results obtained by the authors when repeating them in their charcoal vacua.

By operating with discs of rock-salt and other materials under various circumstances of absorption, the observed phenomena are traced to the unequal heating of the movable parts of the apparatus; and their full explanation is given from the kinetic theory of gaseous pressure.

To confirm this explanation various additional experiments are described—some, in particular, with amorphous sulphur. The amount of radiation from a magnesium lamp, as measured by the pyrheliometer, is shown to be quite consistent with the explanation offered.



ELECTRIC RESISTANCE OF IRON.

[*Deferred from p.* 491.]

On the Electric Resistance of Iron at a High Temperature. By Messrs C. M. Smith, C. G. Knott, and A. Macfarlane. (Plate.)

The following paper is a continuation of a former brief one, communicated to the Society, and printed in the *Proceedings*, on the change of electric resistance of iron due to change of temperature. In a note appended to Prof. Tait's paper on a "First Approximation to a Thermo-electric Diagram" (Trans. R. S.E., 1872–73), attention was drawn to the curious phenomenon observed by Gore, that at a temperature about dull red heat, iron wire undergoes sudden changes in length, and also to the further discovery by Prof. Barrett, that if the wire be cooling, a sudden reglow occurs simultaneously with these changes. These phenomena seemed to be connected with other known physical changes which take place in iron at this critical temperature, such as the loss of its magnetic properties, the remarkable bend of the iron line in the thermo-electric diagram, and the interesting alteration in the rate of change of electric resistance with respect to change of temperature, observable in iron at the same dull red heat. The following experiments were made mainly with the view of more thoroughly investigating this last peculiarity.

The method employed in the first series of experiments consisted in comparing the change of resistance with time, the wire throughout the whole of the experiment being surrounded for the greater part of its length by an iron cylinder which had been previously heated to a white heat in a stove, and was then allowed to cool by radiation. By this means a sufficiently slow and uniform alteration of temperature was secured; and the curve (see diagram, Fig. I.) as plotted in terms of the resistance and the time as ordinate and abscissa, shows the remarkable and sudden change of $\frac{dR}{dt}$ at a temperature about the dull red heat—a change observable in all the experiments in connection with the iron wire. Upon the substitution of an equal length of platinum wire for the iron, *ceteris paribus*, it was found that no similar change was observable—the

curve obtained (see diagram, Fig. II.) being throughout the same range of temperature very accurately a straight line. The resistance was measured by shunting the current in the galvanometer and battery circuit through the wire under consideration. One great disadvantage of this method is, that the curves do not represent strictly the relation between temperature and resistance, since the rate of cooling is not uniform, and the wire is not at one temperature throughout.

In the second method, however, this difficulty was overcome; for time, as a variable, was eliminated by combining the two originally separate experiments with iron and platinum into one, and comparing the simultaneous changes in the resistances of these wires, which were in exactly similar circumstances. Equal lengths of iron and platinum wire were led side by side through the horizontal cylinder, and their extremities were so connected with the galvanometer and battery circuit, that by simply rocking a six-footed rocker, working in six mercury holes, the current could be shunted through each wire alternately, and thus their resistances could be compared by the effects produced upon the galvanometer.

The curves obtained from these experiments with platinum and iron (see diagram, Fig. III.), their indications being here abscissæ and ordinates, show the same marked change at the same critical temperature. When palladium was substituted for platinum the same peculiarity was observable (see diagram, *Fig.* IV.); but when palladium was substituted for iron, the curve obtained (see diagram, Fig. V.) was an accurate straight line. It was found expedient, after the first few preliminary experiments, to introduce into the battery circuit a commutator, by which to reverse the current, and so eliminate all errors referable to thermo-electric effects due to the unequal heating (by radiation from the cylinder, or conduction along the heated wires), of the various metallic junctions in the circuit.

In the third distinct series of experiments the arrangement was more elaborate. To the platinum or other wire, whose resistance was compared with that of iron, was attached at the middle point a third wire. By a mechanical arrangement of rockers and commutators, the battery and iron wire could be thrown out of the galvanometer circuit, and thus a thermo-electric standard of temperature was obtained, with which the resistances of the two

wires could be at any instant compared. One great drawback in all these experiments was the oxidation of the iron wire. In order to get rid of this to some extent, an entirely new arrangement was devised, in which the heating of the wires was effected by the same current which measured the resistance; but the results obtained by this method were far from satisfactory, owing to the many practical difficulties which were continually cropping up. These experiments were conducted during March and April of 1874.

In the following June, experiments similar to those of the third series above mentioned were made with an iron wire and two platinum-iridium alloys—the same which are called M and N in the thermo-electric diagram. The resistances of M were compared with those of iron, and readings as nearly simultaneous as possible were taken of the deflections due to the M–N thermo-electric junction in the manner described above. Immediately upon the completion of this experiment a triple junction was set up of M, N, and the iron wire already used. The currents due to the Fe–M and M–N junctions were then compared, and, from the curve obtained (see diagram, Fig. VI.), which shows the usual parabolic characters at and near the neutral points, the iron line was laid down with reference to N (Fig. VII.). The features of this line, taken in connection with the M–N deflections observed in the "resistance" experiment, conclusively prove that the bend of the iron line in the thermo-electric diagram occurs at almost exactly the temperature at which the sudden change in the otherwise nearly uniform $\frac{dR}{dt}$ of the same iron wire is observable.

In the diagrams of the various experiments, *all* observed points which do not lie exactly on the curves traced have been inserted.

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ERRATA.

Page 549, line 23, *instead of* "Wheatstone's method (in our reference to which Galvanometer is printed Electrometer)," *read:* "the Electrometer method to which we referred."

Page 549, line 26, *instead of* "Wheatstone," *read:* "this method."

Page 550, line 4 from the bottom, *After* "p. 198" *add* "Also Papers on Electrostatics and Magnetism by Sir William Thomson, 1872, § 350."

Page 555, line 7, *after* "original liquid," *read:* "and that there is no special resistance to the passage of the current from the electrode to the liquid?"

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